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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. X.

(FIFTH SERIES).

NUMBER LIX.—JULY 1880.

NUMBER LX.—AUGUST.

NUMBER LXI.—SEPTEMBER.

NUMBER LXII.—OCTOBER.

NUMBER LXIII.—NOVEMBER.

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PLATES.

ERRATA.

Page 122, line 1, *for* $e = \Sigma$ *read* $e = E$.
— 292, line 1, *for* 92,400 calories *read* 29,400 calories.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JULY 1880.

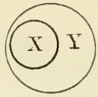
I. *On the Diagrammatic and Mechanical Representation of Propositions and Reasonings.* By J. VENN, *M.A., Fellow and Lecturer in Moral Science, Caius College, Cambridge**.

SCHEMES of diagrammatic representation have been so familiarly introduced into logical treatises during the last century or so, that many readers, even of those who have made no professional study of logic, may be supposed to be acquainted with the general nature and object of such devices. Of these schemes one only, viz. that commonly called "Eulerian circles," has met with any general acceptance. A variety of others indeed have been proposed by ingenious and celebrated logicians, several of which would claim notice in a historical treatment of the subject; but they mostly do not seem to me to differ in any essential respect from that of Euler. They rest upon the same leading principle, and are subject all alike to the same restrictions and defects.

Euler's plan was first proposed by him† in his 'Letters to a German Princess,' in the part treating of logical principles and rules. What we here represent is, of course, the extent or scope of each term of the proposition. We draw two circles, and make them include or exclude or intersect one another, according as the classes denoted by the terms happen to stand in relation to one another in this respect. Thus "All

* Communicated by the Author.

† According to Drobisch and Ueberweg, this circular device had been already proposed by two previous writers, viz. C. Weise and J. C. Lange.

X is Y" is represented in the form  ; "No X is Y" is represented  . When two propositions are to be combined into a syllogism, *three* circles are of course thus introduced, the mutual relations of the first and third being determined by their separate relations to the second.

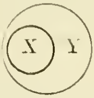
In spite of certain important and obvious recommendations about this plan, it seems to me to labour under two serious defects, which indeed prevent its effective employment except in certain special cases.

In the first place, then, it must be noticed that these diagrams do not naturally harmonize with the propositions of ordinary life or ordinary logic. To discuss this point fully would be somewhat out of place here; and as I have entered rather minutely into the question in a journal devoted to speculative inquiry*, I will confine myself to a very short statement. The point is this. The great bulk of the propositions which we commonly meet with are founded, and rightly founded, on an imperfect knowledge of the actual mutual relations of the implied classes to one another. When I say that all X is Y, I simply do not know, in many cases, whether the class X comprises the whole of Y or only a part of it. And even when I do know how the facts are, I may not intend to be explicit, but may purposely wish to use an expression which leaves this point uncertain. Now one very marked characteristic about these circular diagrams is that they forbid the natural expression of such uncertainty, and are therefore only directly applicable to a very small number of such propositions as we commonly meet with. Accordingly, if we resolve to make use of them, we must do one of three things. Either we must confine ourselves to propositions which are actually explicit in this respect, or in which the data are at hand to make them explicit—such as "X and Y are coextensive," "Some only of the X's are to be found amongst the Y's," and so forth; or we must feign such a knowledge where we have it not, which would of course be still more objectionable; or we must offer an *alternative choice* of diagrams, admitting frankly that, though one of these must be appropriate to the case in question, we cannot tell which it is. This third is the only legitimate course, and in the case of very simple propositions it does not lead to much intricacy; but when we have to combine groups of propositions, the

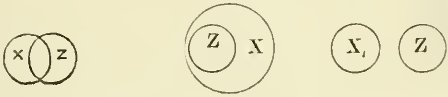
* 'Mind,' No. xix., July 1880.

number of possible resultant alternatives would be very considerable.

For instance, the proposition "All X is Y" needs *both* the diagrams, 



; for we cannot tell, from the mere verbal statement, whether there are any Y's which are not X. Similarly the proposition "Some X is not Z" needs *three* other diagrams,



(These five relations, it may be remarked, comprise all the possible ways in which two terms may stand to one another.) Hence the combination of the two given premises could not be adequately represented by less than six figures. If more premises, and more complicated ones (such as we shall presently proceed to illustrate), are introduced, the consequent confusion would be very serious. The fact is, as I have explained at length in the article above referred to, that the five distinct relations of classes to one another (viz. the inclusion of X in Y, their coextension, the inclusion of Y in X, their intersection, and their mutual exclusion), which are thus pictured by these circular diagrams, rest upon a totally distinct view as to the import of a proposition from that which underlies the statements of common life and common logic. The latter statements naturally fall into four forms—the universal and particular, affirmative and negative; and it is quite impossible to make the five divisions of the one scheme fit in harmoniously with the four of the other.

The second objection to which this scheme is obnoxious is of a more practical character; and viewed in that light it is, if any thing, of a still more serious character. It consists in the fact that we cannot readily break up a complicated problem into successive steps which can be taken independently. We have, in fact, to solve the problem first, by determining what are the actual mutual relations of the classes involved, and then to draw the circles to represent this final result; we cannot work step by step towards the conclusion by aid of our figures.

The extremely simple examples afforded by the syllogism do not bring out this difficulty; and it is consequently very apt to be overlooked. Take, for instance, the pair of propositions, "No Y is Z," "All X is Y." Here we have the relation of X to Y, and of Y to Z, given independently of one another; and

this immensely simplifies the problem. We can think of each pair of circles without troubling ourselves about the other pair; we have nothing resembling *implicit* equations. But suppose that, on the other hand, we had a statement of the relation of X to Y and Z combined with others giving that of Y to Z and W, and, say, X to W, we should hardly know where to begin. Each statement being interlinked with the others, no one of them could be disentangled and represented separately. No doubt when the problem had been solved somehow, and a full determination secured of the mutual relations of the various classes, we could then set about undertaking to draw our circles. But this is a very different thing from working by help of the diagrams and employing them to aid our conceptions in the actual task of solution. The simple fact is that on this scheme, as already remarked, we have no means of exhibiting imperfect knowledge. What is exhibited is the *final outcome* of the relation, the actual exclusion or inclusion of the classes; and consequently we cannot represent our partial knowledge or the steps by which we attain to complete information. This defect comes out even in such a simple case as the ordinary disjunctive proposition "Every X is either Y or Z." Such a statement gives us no information as to the mutual relations of Y to Z; and therefore, since we have no means of marking by aid of our circles any thing but the actual relations of these classes, we should have to draw out a complete scheme of all the possibilities. This would demand, to begin with, five different figures displaying the five possible relations of Y to Z. We should then have to proceed to draw our X circle in each case, applying it as well as we could to each of these different figures. It will not need a moment's consideration to realize how tedious and complicated such a process would soon become when several class terms have thus to be combined.

We must therefore cast about for some new scheme of diagrammatic representation which shall be competent to indicate imperfect knowledge on our part; for this will at once enable us to appeal to it step by step in the process of working out our conclusions. I have never seen any hint at such a scheme, though the want seems so evident that one would suppose that something of the kind must have been proposed before. The one here offered may be said to underlie Boole's method *, and to be the appropriate diagrammati

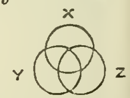
* I tried at first, as others have done, to represent the complicate propositions, there introduced, by the old plan; but the representation failed altogether to answer the desired purpose; and after some conside ration I hit upon the plan here described.

representation for it. He makes no employment of diagrams himself, nor any suggestion for them.

One essential characteristic of Boole's method, as many readers of this article will probably know, is the complete subdivision of our field of inquiry into all the elementary classes which can possibly be yielded by combination of all the terms involved. Let there be two terms, X and Y; then we have to take account of the four subclasses, X that is Y, X that is not Y, Y that is not X, and what is neither X nor Y. Writing, for simplicity, $\bar{X}$ for not-X, the four classes are XY, X$\bar{Y}$, $\bar{X}$Y, $\bar{X}\bar{Y}$. Three class terms similarly yield eight subclasses, which admit of equally ready symbolic representation, and so on. Generally, if there be n classes involved in any given combination of logical premises there will be 2^n subclasses, every one of which must, somehow or other, be taken account of in any complete investigation of the problem.

This consideration seems to suggest a more hopeful scheme of diagrammatic representation. Whereas the Eulerian plan endeavoured at once and directly to represent *propositions*, or relations of class terms to one another, we shall find it best to begin by representing only *classes*, and then proceed to modify these in some way so as to make them indicate what our propositions have to say. How, then, shall we represent all the subclasses which two or more class terms can produce? Bear in mind that what we have to indicate is the successive duplication of the number of subdivisions produced by the introduction of every successive term, and we shall see our way to a very important departure from the Eulerian conception. All that we have to do is to draw our figures, say circles, so that each successive one which we introduce shall intersect once, and once only, all the subdivisions already existing, and we then have what may be called a general framework indicating every possible combination producible by the given class terms. This successive duplication of the number of subclasses was the essential characteristic when we were dealing with such symbols as X and Y. For suppose these two terms only involved, and there resulted the four minor classes indicated by XY, X$\bar{Y}$, $\bar{X}$Y, and $\bar{X}\bar{Y}$. Now suppose that a third term Z makes its appearance. This at once calls for a subdivision of each of these four into its Z and $\bar{Z}$ parts respectively. Thus XY is split up into XYZ and XY$\bar{Z}$, and so with the others, whence we get the eight subdivisions demanded. Provided our diagrams represent this characteristic clearly and unambiguously, they will do all that we can require of them.

The leading conception of this scheme is then simple enough; but it involves some consideration in order to decide upon the most effective and symmetrical plan of carrying it out. Up to three terms, indeed, there is but little opening for any difference; and as the departure from the familiar Eulerian plan has to be made from the very first, we will examine these simpler cases somewhat carefully. The diagram for two terms, then, is to be thus drawn :— 

On the common plan this would represent a *proposition*, and is, indeed, very commonly taken as illustrative of the proposition "Some X is Y."* With us it does not as yet represent a proposition at all, but only the framework into which propositions can be fitted; that is, it represents only the four combinations indicated by the letter-compounds XY, $X\bar{Y}$, $\bar{X}Y$, $\bar{X}\bar{Y}$. Now conceive that we have to reckon also with the presence, and consequently with the absence, of Z. We just draw a third circle intersecting the two above, thus, 

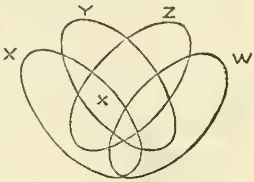
, and we have the eight compartments or classes which we need. The subdivisions thus produced correspond precisely with the letter-combinations. Quote one of these latter, and the appropriate class-division is ready to meet it; put a finger on any compartment, and the letter indication is unambiguous. Moreover both schemes, that of letters and that of spaces, agree in being mutually exclusive and collectively exhaustive in respect of all their elements. No one of the elements trespasses upon the ground of any other; and amongst them they account for all possibilities. Either scheme, therefore, may be taken as a fair representative of the other.

Beyond three terms circles fail us, since we cannot draw a fourth circle which shall intersect three others in the way required. But there is no theoretic difficulty in carrying out the scheme indefinitely. Of course any closed figure will do as well as a circle, since all that we demand of it, in order that it shall adequately represent the contents of a class, is that it shall have an inside and an outside, so as to indicate what does and what does not belong to the class. There is nothing to prevent us from going on for ever thus drawing successive figures, doubling the consequent number of subdivisions. The only objection is, that since diagrams are pri-

* It really takes, however, *three* common propositions to exhaust its significance; for the figure involves in addition the two statements "Some X is not Y," and "Some Y is not X."

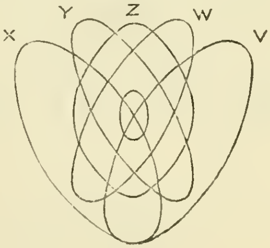
marily meant to assist the eye and the mind by the intuitive nature of their evidence, any excessive complication entirely frustrates their main object.

For four terms the simplest and neatest figure seems to me to be one composed of four equal ellipses thus arranged:—



It is obvious that we thus get the sixteen compartments that we want, counting, as usual, the outside of them all as one compartment. The eye can distinguish any one of them in a moment by following the outlines of the various component figures. Thus the one which is asterisked is instantly seen to be "X that is Y and Z, but is not W," or $X Y Z \bar{W}$; and similarly with any of the others. The desired condition that these sixteen alternatives shall be mutually exclusive and collectively exhaustive, so as to represent all the component elements yielded by the four terms taken positively and negatively, is of course secured.

With five terms ellipses fail, at least in the above simple form. It would be quite possible to sketch out figures of a somewhat horse-shoe shape which should answer the purpose—that is, five of which should fulfil the condition of yielding the desired thirty-two distinctive and exhaustive compartments. For all practical purposes, however, any outline which is not very simple and easy to follow with the eye, fails entirely in its main purpose of affording intuitive and sensible illustration. What is wanted is that we should be able to distinguish and identify any assigned compartment in a moment, so as to see how it lies in respect of being inside and outside each of the principal component figures. For this purpose, when five class terms are introduced, I do not think that any arrangement will much surpass the following (the small ellipse in the centre is here to be reckoned as a piece of the *outside* of Z; *i. e.* its four component portions are inside of Y and W, but are no part of Z).

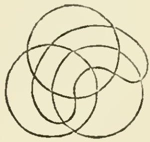


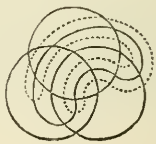
It must be admitted that such a diagram is not quite so simple to draw as one might wish it to be; but then we must remember what are the alternatives before any one who wishes to grapple effectively with five terms and all the thirty-two possibilities which they yield. He must either write down or in some

way or other have set before him all those thirty-two compounds of which X Y Z W V is a sample; that is, he must contemplate the array produced by 160 letters. In comparison with most ways of doing *that*, the sketching out of such a figure is a pleasure, besides being far more expeditious; for, with a very little practice, any of the diagrams here offered might be drawn in but a minute fraction of the time requisite to write down all the letter-compounds. I can only say for myself that, after having for various purposes worked through hundreds of logical examples, I generally resort to diagrams of this description; it not only avoids a deal of unpleasant drudgery, but is also a valuable security against error and oversight. The way in which this last advantage is secured will be best seen presently, when we come to inquire how these diagrams are to be used to represent propositions as distinguished from mere terms or classes.

Beyond five terms it hardly seems as if diagrams offered much substantial help; but then we do not often have occasion to meddle with problems of a purely logical kind which involve such intricacies. If we did have such occasion, viz. to visualize the sixty-four compounds yielded by the six terms X, Y, Z, W, V, U, the best plan would probably be to take two of the above five-term figures—one for the U part and the other for the not-U part of all the other combinations. This would yield the desired distinctive sixty-four subdivisions, but, of course, it to some extent loses the advantage of the *coup d'œil* afforded by a single figure.

We have endeavoured above to employ only symmetrical figures, such as should not merely be an aid to the sense of sight, but should also be to some extent elegant in themselves. But for merely theoretical purposes the rule of formation would be very simple. It would merely be to begin by drawing any closed figure, and then proceed to draw others, subject to the one condition that each is to intersect once and once only all the existing subdivisions produced by those which had gone before. Proceeding thus we should naturally select circles as the simplest figures, so long as they would answer our purpose; that would be, up to three terms inclusive. The two successive modifications, aiming always at simplicity of figure, would then be naturally such as the following (the fifth figure is marked, for clearness, by a dotted line):—





A number of deductions will occur to the logical reader which it may be left to him to work out. Some of them may be just indicated. For instance, any two compartments between which we can communicate by crossing only one line, can differ by the affirmation and denial of one term only, *ex. gr.* $X Y Z W$ and $X Y \bar{Z} W$. Accordingly, when two such are compounded, or, as we may say, "added" together, they may be simplified by the omission of such term; for the two together make up all $X Y W$. Any compartments between which we can only communicate by crossing two boundaries, *ex. gr.* $X Y \bar{Z} W$ and $X \bar{Y} Z W$, must differ in two respects; it would need *four* such compartments to admit of simplification, the simplification then resulting in the opportunity of dropping the reference to *two* terms; *ex. gr.* $X Y \bar{Z} W$, $X \bar{Y} Z W$, $X Y Z W$, $X \bar{Y} \bar{Z} W$, taken together lead simply to $X W$. Many similar suggestions will present themselves.

So far, then, this diagrammatic scheme has only been described as representing terms or classes; we have now to see how it can be applied so as to represent propositions. Before doing this it will be necessary to indicate a certain view as to the Import of Propositions, because it is one which is not familiar or generally accepted, though it is very relevant and important for our present purpose. That view is briefly this —that every universal proposition, whether or not it be originally stated in a negative form, may be adequately represented by one or more *negations*. To give a complete justification of this view would involve a discussion which would be quite unsuitable to a general article like this; but a very few remarks will serve to explain, and to a considerable extent to justify it.

For instance, the common proposition "No X is Y," will be read as just denying the existence of the combination $X Y$, and therefore needs but little alteration. The proposition "All X is Y" will be read as denying the combination "X that is not Y" or $X \bar{Y}$; and the destruction of that combination will here be regarded as its full import. "X is either Y or Z" will be considered fully accounted for when we have said that it denies "X that is neither Y nor Z" or $X \bar{Y} \bar{Z}$. "Every X that is not Y must be both Z and W" destroys the two combinations $X \bar{Y} \bar{Z}$ and $X \bar{Y} \bar{W}$, and so on. In a full exposition of the method here indicated, rules might conveniently be given for thus breaking up complex propositions into all the elementary denials which they implicitly contain; but the exercise of ordinary ingenuity will quite suffice thus

to interpret any of the premises which we propose to take account of.

Another way of approaching the same question is by inquiring whether the various subdivisions in our diagram are to be considered as representing *classes*, or merely *compartments* into which classes may or may not have to be put. The latter view must be accepted as being the only one with which we can conveniently work. We may doubtless regard them as representing classes; but if we do so, we must keep in mind the proviso "if there be such a class of things in existence." And when this condition is insisted on, we appear to express our meaning best by saying that what our diagrammatic subdivisions (or, for that matter, the corresponding literal symbols) stand for are compartments which may or may not happen to be occupied.

One main reason for insisting upon this point is to be found in the impossibility of ascertaining, until we have fully analyzed our premises, whether or not any particular combination is possible. In the simple propositions of the common logic this difficulty hardly occurs; so that when we say "All X is Y," we take it for granted, or are apt to do so, that there must be both X's and Y's to be found. But if this proposition, or, still more, a complicated one of the same type, occurred as one of a group of premises, matters would be very different. We should then find that to *maintain* the existence of all the subjects and predicates, instead of merely *denying* the existence of the various combinations *destroyed* by them, would sadly hamper us in our interpretation of groups of premises *.

Take, for instance, the following group of premises, which are by no means of a very complicated nature:—

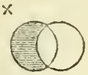
All X is either both Y and Z or not-Y,
All XY that is Z is also W,
No WX is YZ.

It would not be easy to detect, from mere contemplation of these data, that though they admit the possible existence of such classes as XZ and YZ, they deny that of the class XY. But since, as they stand, XY is the subject of one of them, we could not consistently admit such a conclusion unless we restricted the force of that second premise to what it *denies*, viz.

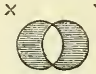
* I am not aware that it has ever been maintained that such a group of elementary denials is to be regarded as an *adequate* interpretation of these propositions. But it seems quite clear to me (on grounds too intricate to enter upon here) that this is the view which must be considered to underlie Boole's system, and, indeed, any general symbolic system of logic, if it is to be worked successfully.

by saying that it just destroys the class $XYZ\bar{W}$ or "X that is Y and Z but not W," and does nothing else*.

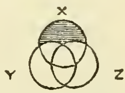
The method of employing the diagrams in order to express propositions will readily be understood. It is merely this :— Ascertain what each given proposition denies, and then put some kind of mark upon the corresponding partition in the figure. The most effective means of doing this is just to shade it out. For instance, the proposition "All X is Y" is interpreted to mean that there is no such class of things in existence as "X that is not-Y" or $X\bar{Y}$. All, then, that we have to do is to scratch out that subdivision in the two-circle figure, thus,



. If we want to represent "All X is all Y," we take this as adding on another denial, viz. that of $\bar{X}Y$, and we proceed to scratch out that division also, thus,



The main characteristic of this scheme, viz. the facility with which it enables us to express each separate accretion of knowledge, and so to break up any complicated group of data, and attack them in detail, will begin to show itself even in such a simple instance as this. On the common plan we should have to begin again with a new figure in each case respectively, viz. for "All X is Y," and "All X is all Y;" whereas here we use the same figure each time, merely modifying it in accordance with the new information. Or take the disjunctive "All X is either Y or Z." It is very seldom even attempted to represent this diagrammatically (and then, so far as I have seen, only if the alternatives are mutually exclusive); but it is readily enough exhibited when we regard it as merely extinguishing any X that is neither Y nor Z—thus,



If to this were added the statement that "none but the X's are either Y or Z," we should then abolish the $\bar{X}Y$ and the $\bar{X}Z$, and have

. Scratch out, again, the XYZ compart-

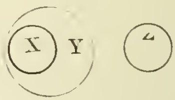
* Though this interpretation, however, of the import of propositions seems desirable for a really generalized system of logic, it is by no means necessary to adopt it in order to explain and justify the use of the diagrammatic method here proposed.

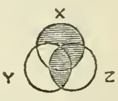
ment, and we have made our alternatives exclusive; *i. e.* the X is then Y or Z *only*.

Of course the same plan is easy to adopt with any number of premises. Our first data abolish, say, such and such classes. This is final; for, as already intimated, all the resultant elementary denials which our propositions yield must be regarded as absolute and unconditional. This first step then leaves the field open to any similar accession of knowledge from the next data; and so more classes are swept away. Thus we go on till all the data have had their fire; and the muster-roll at the end will show what classes may be taken as surviving. If, therefore, we simply shade out the compartments in our figure which have thus been successively proved to be empty, nothing is easier than to go on doing this till all the information yielded by the data is exhausted. In doing this it may, of course, often happen that some of the data wholly or partially go over the same ground as others. In that case, whichever of such data is considered after the other, finds its work already done for it entirely or in part; the class which we were going to mark for destruction is found to be already gone, and there is nothing to do so far as it is concerned.

As the syllogistic figures are the form of reasoning most familiar to ordinary readers, I will begin with one of them, though they are too simple to serve as effective examples. Take, for instance,

No Y is Z,
All X is Y,
∴ No X is Z.

This would commonly be exhibited thus, .

It is easy enough to do this; for in drawing our circles we have only to attend to two terms at a time, and consequently the relation of X to Z is readily detected; there is not any of that troublesome interconnexion of a number of terms simultaneously with one another which gives rise to the main perplexity in complicated problems. Accordingly such a simple example as this is not a very good one for illustrating the method now proposed; but, in order to mark the distinction, the figure to represent it is given, thus, 

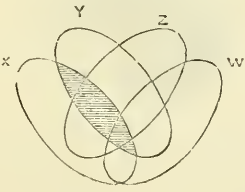
In this case the one particular relation asked for, viz. that of X to Z, it must be admitted, is not made more obvious on

our plan than on the old one. The superiority, if any, in such an example must rather be sought in the completeness of the pictorial information in other respects—as, for instance, that, of the four kinds of X which may have to be taken into consideration, one only, viz. the $XY\bar{Z}$, or the "X that is Y but is not Z," is left surviving. Similarly with the possibilities of Y and Z: the relative number of these, as compared with the actualities permitted by the data, are detected at a glance.

As a more suitable example consider the following—

$$\begin{cases}\text{All X is either Y and Z, or not-Y,} \\ \text{If any XY is Z, then it is W,} \\ \text{No WX is YZ;}\end{cases}$$

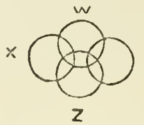
and suppose we are asked to exhibit the relation of X and Y to one another as regards their inclusion and exclusion. The problem is essentially of the same kind as the syllogistic one; but we certainly could not draw the figures in the same offhand way we did there. Since there are four terms, we sketch the appropriate 4-ellipse figure, and then proceed to analyze the premises in order to see what classes are destroyed by them. The reader will readily see that the first premise annihilates all "XY which is not Z," or $XY\bar{Z}$; the second destroys "XYZ which is not W," or $XYZ\bar{W}$; and the third "WX which is YZ," or WXYZ. Shade out these three classes, and we see the resultant figure at once, viz.



It is then evident that *all* XY has been thus made away with; that is, X and Y must be mutually exclusive, or, as it would commonly be thrown into propositional form, "No X is Y."

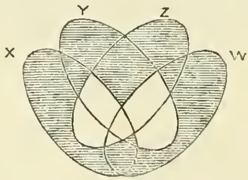
I will not say that it would be impossible to draw Eulerian circles to represent all this, just as we draw them to represent the various moods of the syllogism; but it would certainly be an extremely intricate and perplexing task to do so. This is mainly owing to the fact already alluded to, viz. that we cannot break the process up conveniently into a series of easy steps each of which shall be complete and accurate as far as it goes. But it should be understood that the failure of the older method is simply due to its attempted application to a some-

what more complicated set of data than those for which it was designed. But these data are really of the same kind as when we take the two propositions "All X is Y," "All Y is Z," and draw the customary figure. *When* the problem, however, *has been otherwise solved*, it is easy enough to draw a figure of the old-fashioned, or "inclusion-and-exclusion" kind, to represent the result, as follows,



; but one may safely assert that not many persons would have seen their way to drawing it at first hand for themselves*.

One main source of aid which diagrams can afford is worth noticing here. It is that sort of visual aid which is their especial function. Take the following problem:—"Every X is either Y or Z; every Y is either Z or W; every Z is either W or X; and every W is either X or Y: what further condition, if any, is needed in order to ensure that every XY shall be W?" It is readily seen that the first statement abolishes any X that is neither Y nor Z, and similarly with the others; so that the four abolished classes are $X\overline{YZ}$, $Y\overline{ZW}$, $Z\overline{WX}$, and $W\overline{XY}$. Shade them out in our diagram, and it stands thus:—



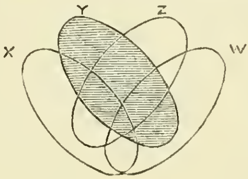
It is then obvious that, of the surviving component parts of XY, one only (viz. $XYZ\overline{W}$) is not W. If, then, this be destroyed, *all* XY will be W; that is, the necessary and sufficient condition is that "all XYZ is W."

* Even then we have said more in this figure than we are entitled to say. For instance, we have implied that there *is* some X which is W, and so forth. The other scheme does not thus commit us; for though the extinction of a class is final, its being let alone merely spares it conditionally. It holds its life subject to the sentence, it may be, of more premises to come. This must be noticed, as it is an important distinction between the customary plan and the one here proposed. The latter makes the distinction between rejection and non-rejection—such non-rejection being provisional, and not necessarily indicating ultimate acceptance. The former has to make the distinction between rejection and acceptance; for the circles must either intersect or not, and their non-intersection indicates the definite abandonment of the class common to both. Hence the practical impossibility of appealing to such diagrams for aid in representing complicated groups of propositions.

In the same way the implied total abolition of any one class is thus made extremely obvious. Take, for example, the following premises, and let us ask quite generally for any obvious conclusion which follows from them:—

$$\begin{cases}\text{Every Y is either X and not Z, or Z and not X;}\\ \text{Every WY is either both X and Z, or neither of the two;}\\ \text{All XY is either W or Z, and all YZ is either X or W.}\end{cases}$$

It will be seen on reflection that these statements involve respectively the abolition of the following classes, viz.:—(1) of YXZ, $Y\bar{X}\bar{Z}$; (2) of $WYX\bar{Z}$ and $WY\bar{X}Z$; (3) of $XY\bar{W}\bar{Z}$, $YZ\bar{X}\bar{W}$. Shade out the corresponding compartments in the diagram, and it presents the following appearance—



It is then clear at a glance that the collective effect of the given premises is just to deny that there can be any such class of things as Y in existence, though they leave every one of the remaining eight combinations perfectly admissible. This, then, is the diagrammatic answer to the proposed question.

It will be easily seen that such methods as those here described readily lend themselves to mechanical performance. I have no high estimate myself of the interest or importance of what are sometimes called logical machines, and this on two grounds. In the first place, it is very seldom that intricate logical calculations are practically forced upon us; it is rather we who look about for complicated examples in order to illustrate our rules and methods. In this respect logical calculations stand in marked contrast with those of mathematics, where economical devices of any kind may subserve a really valuable purpose by enabling us to avoid otherwise inevitable labour. Moreover, in the second place, it does not seem to me that any contrivances at present known or likely to be discovered really deserve the name of logical machines. It is but a very small part of the entire process which goes to form a piece of reasoning which they are capable of performing. For, if we begin from the beginning, that process would involve four tolerably distinct steps. There is, first, the statement of our data in accurate logical language. This step deserves to be reckoned, since the variations of popular language are so

multitudinous, and often so vague and ambiguous, that they may need careful consideration before they can be reduced to form. Then, secondly, we have to throw these statements into a form fit for the engine to work with—in this case the reduction of each proposition to its elementary denials. It would task the energies of a machine to deal at once, say, with all the premises employed even in the few examples here offered. Thirdly, there is the combination or further treatment of our premises after such reduction. Finally, the results have to be interpreted or read off. This last generally gives rise to much opening for skill and sagacity; for though in such examples as the last (in which one class, Y, was simply abolished) there is but one answer fairly before us, yet in most cases there are many ways of reading off the answer. It then becomes a question of judgment which of these is the simplest and best. For instance, in the last example but one, there are a quantity of alternative ways of reading off our conclusion; and until this is done the problem cannot be said to be solved. I cannot see that any machine can hope to help us except in the third of these steps; so that it seems very doubtful whether any thing of this sort really deserves the name of a logical engine.

It may also be remarked that when we make appeal, as here, to the aid of diagrams, the additional help to be obtained by resort to any kind of mechanical contrivance is very slight indeed. So very little trouble is required to sketch out a fresh diagram for ourselves on each occasion, that it is really not worth while to get a machine to do any part of the work for us. Still as some persons have felt much interest in such attempts, it seemed worth while seeing how the thing could be effected here. There is the more reason for this, since the exact kind of aid afforded by mechanical appliances in reasoning, and the very limited range of such aid, do not seem to be generally appreciated.

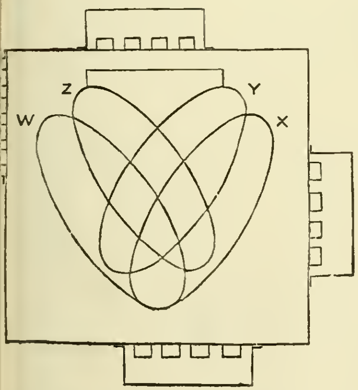
For myself, if I wanted any help in constructing or employing a diagram, I should just have one of the three-, four-, or five-term figures made into a stamp; this would save a few minutes sometimes in drawing them; and we could then proceed to shade out or otherwise mark the requisite compartments. More help than this would be of very little avail. However, since this is not exactly what people understand by a logical machine, I have made two others, in order to give practical proof of feasibility.

For instance, a plan somewhat analogous, I apprehend, to Prof. Jevons's *abacus* would be the following:—Have the desired diagram (say the five-term figure with its thirty-two compartments) drawn on paper and then pasted on to thin

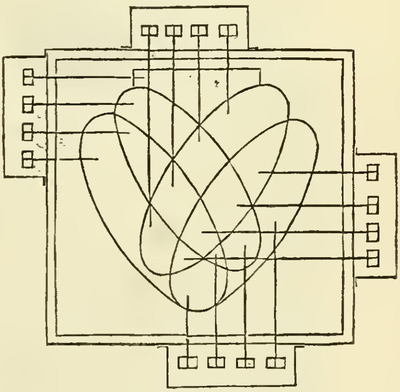
board. Cut out all the subdivisions by following the lines of the different figures, after the fashion of the children's maps which are put together in pieces. The corresponding step to shading out any compartment would then be the simple removal of the piece in question. We begin with all the pieces arranged together, and then pick out and remove those which represent the non-existent classes. When every one of the given premises has thus had its turn, the pieces left behind will indicate all the remaining combinations of terms which are consistent with the data. I have sometimes found it convenient, where the saving of a little time was an object, to use a contrivance of this kind. There is no reason to give a drawing of it, since any one of the figures we have hitherto employed may really be regarded as such a drawing.

Again, corresponding to Prof. Jevons's logical machine, the following contrivance may be described. I prefer to call it merely a logical-diagram machine, for the reasons already given; but I suppose that it would do very completely all that can be rationally expected of any logical machine. Certainly, as regards portability, nothing has been proposed to equal it, so far as I know; for though needlessly large as made by me, it is only between five and six inches square and three inches deep. It is intended to work for four terms; and the following figures will serve to show its construction:—

I.



II.



The first figure represents the upper surface of the instrument. It shows the diagram of four elipses, the small irregular compartment at the top of them being a representative part of the outside of all the four class-figures; that is, this com-

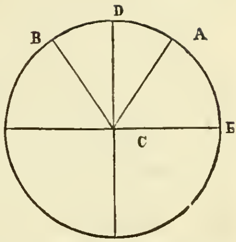
partment stands for what is neither X, Y, Z, nor W, or $\overline{X}\,\overline{Y}\,\overline{Z}\,\overline{W}$. The second figure represents a horizontal section through the middle of the instrument. Each of the ellipses here is, in fact, a section of an elliptical cylinder, these cylinders intersecting one another so as to yield sixteen compartments. Each compartment has a wooden plug half its height, which can move freely up and down in the compartment. When the machine is ready for use each plug stands flush with the surface, being retained there by a pin; we therefore have the appearance presented in fig. I. When we wish to represent the destruction of any class, all we have to do is slightly to draw out the appropriate pin (the pins of course are duly labelled, and will be found to be conveniently grouped), on which the plug in question drops to the bottom. This, of course, is equivalent to the shading of a subdivision in the plane diagram. As the plugs have to drop independently of one another, a certain number of them, it will be seen, have to have a slot cut in them, so as to play free from the pins belonging to other plugs. When the plugs have to be returned to their places at the top, all we have to do is to turn the instrument upside down, when they instantly fall back, and on pressing in the pins again they are retained in their place. The guards outside the pins are merely to prevent them from being drawn entirely out.

II. *On a simple Form of Saccharimeter. By* J. H. POYNTING, *Fellow of Trinity College, Cambridge, Professor of Physics in Mason's College, Birmingham**.

THE general principle of the modification of the saccharimeter which I shall describe in this paper is well known, and has already been applied in the construction of several standard instruments, such as Jellett's and Laurent's. This principle consists in altering the pencil of rays proceeding from the polarizer in such a way that, instead of the whole pencil having the same plane of polarization, the planes of the two halves are slightly inclined to each other. The analyzer is therefore not able to darken the whole field of view at once. In one position of the analyzer the one half of the field is quite dark; in another position, slightly different, the other half is dark; while when the analyzer is halfway between these two positions, the two halves of the field are equally illuminated. This will be seen from the accompanying figure.

* Communicated by the Physical Society.

Let C A be the trace of the plane of polarization of the right half of the pencil, and C B that of the other half. Let C D bisect A C B. Then, if C E represent the plane of polarization of the light which alone the analyzer will allow to pass, when the analyzer is turned so that C E is perpendicular to C A the right-hand side of the field is dark. When C E is perpendicular to C B the right-hand is partially illuminated (as C A has a component along C E), while the left-hand is dark. Halfway between these positions, when C E is perpendicular to C D, both sides appear equally illuminated. The analyzer being turned round till this equality of illumination is obtained, its position is noted on the attached circle. When an active substance is now inserted in the path of the rays, the planes C A, C B are both rotated through the same angle, and the analyzer has to be rotated through this angle to give the equal illumination once more. The circle again being read, the difference of readings gives the rotation due to the interposed substance.



In Jellett's saccharimeter the inclination of the planes of polarization of the two halves of the field is obtained by interposing a prism of Iceland spar. This is formed by cutting a rhomb nearly parallel to its optic axis, reversing one of the pieces, and then cementing the two together again with the plane of separation bisecting the pencil of rays.

In Laurent's instrument, for which homogeneous light is used, half the pencil is passed through a plate of quartz cut with its axis in the surface and parallel to its edge, the thickness being such that the extraordinary is retarded half a wavelength behind the ordinary. On emergence the directions of vibration in the two parts of the pencil, one of which has traversed the quartz, are equally inclined to the edge of the crystal. The inclination of the two to each other can be very easily altered by simply turning the polarizer.

The following arrangement is in place of the Iceland spar in Jellett's instrument, and of the quartz plate in Laurent's. It seems to be somewhat simpler, and gives fairly good results.

A circular plate of quartz cut perpendicular to the axis is divided along a diameter, and one half slightly reduced in thickness. The two halves are then reunited and interposed in the path of the pencil and at right angles to its direction. Since one half of the pencil passes through a slightly greater thickness of quartz, its plane of polarization is slightly more rotated than that of the other half; and the pencil therefore

emerges with the planes of polarization of its two halves slightly inclined to each other. It is of course always necessary to use homogeneous light to avoid dispersion.

Mr. Glazebrook has very kindly given me the following numbers, which are taken at random from a large number of sets of readings he has obtained for the electromagnetic rotation of certain solutions of NaCl in water; the difference of thickness of the two plates being ·1 mm., and the inclination of the planes of polarization being therefore about 2° for the sodium-light used. The circle to which the analyzer was attached reads to 3′; but the vernier divisions can easily be further subdivided by eye.

	Circle-readings.
I. Current direct.	23° 45′
	23 46
	23 45
	23 45
Current reversed.	21 36
	21 34
	21 39
II. Current direct.	23 15
	23 16
	23 18
Current reversed.	22 19
	22 19
	22 20
III. Current direct.	23 30
	23 30
	23 28
Current reversed.	21 45
	21 47
	21 48

In order to vary the inclination of the two planes of polarization to each other, one of the halves of the quartz plate might be arranged like a Babinet's compensator, so that the difference of the two might be varied at will. The chief objection to the method seems to be that the quartz plate has to be adjusted very exactly perpendicular to the axis of the pencil.

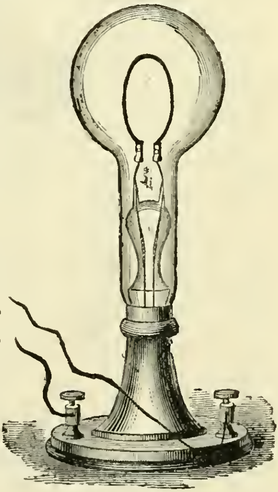
A still simpler arrangement, which has yet only been tried in a somewhat rough form, consists in a cell containing some

active liquid, say sugar solution. This cell is interposed in the path of the pencil; and in it is inserted a piece of plate glass several millims. thick, arranged so that one half the pencil passes through it. This half therefore passes through a less thickness of the active substance than the other half, and is less rotated. The two then emerge as before, having their planes of polarization slightly inclined to each other. This inclination, and consequently the sensitiveness of the instrument can be varied either by varying the strength of the active solution, or the thickness of the plate of glass inserted in the cell.

This arrangement, as far as it has been tested, gives as good results as the previous one, while it is much more easily constructed and adjusted.

III. *Measurements of an Incandescent Paper-carbon Horseshoe Lamp constructed by* Mr. T. A. Edison. *By* Henry Morton, *Ph.D.*, A. M. Mayer, *Ph.D.*, *and* B. F. Thomas, *A.B.**

It may seem almost superfluous to describe the carbon horseshoe electric lamp as recently constructed by Mr. T. A. Edison, so much has been written about it in journals of all descriptions from the daily papers upwards; but to make our work complete we will state briefly that the lamp measured by us, and represented in the accompanying cut, consists of a pear-shaped glass globe with two reentering tubes at its smaller end, through which are passed platinum wires with little screw-clamps at their upper ends, which hold the ends of the carbon horseshoe.



This horseshoe is 1·18 inch high, and 0·72 inch across at the widest part.

It is made by charring a piece of thin cardboard of similar shape, out of contact with air. The interior of the globe is very perfectly exhausted. Fine copper wires connect the platinum wires with the binding-screws on the wooden base of the lamp.

The present writers believe that the following measurements,

* Communicated by the Authors.

made by them in the physical laboratory of the Stevens Institute of Technology, possess some general interest as being the first full and accurate series of determinations, giving the fundamental properties of one of these instruments.

The lamp in question was one of the paper-horseshoe style, No. 154, given by Mr. Edison to the editors of the 'Scientific American,' and by them kindly loaned to us.

We have failed to obtain other lamps directly from Mr. Edison, seemingly because of the offence taken at Menlo Park to the emphatic contradiction which one of us thought it right to give at the very outset to the unfounded claims for Mr. Edison's lamp, which were then published by some of the daily papers.

The lamp here described is certainly a fair specimen of the type to which it belongs, as appears from a general comparison of results with those obtained by the scientific men who recently measured a number of these lamps at Menlo Park under the auspices of Mr. Edison himself.

The work herein described has been in progress for nearly two months, being frequently interrupted by the pressure of other engagements.

Our experiments naturally divide themselves into three groups:—

I. Determination of resistance of lamp as compared with luminous power and with total heat developed.

II. Determination of average of light given out by lamp in all azimuths.

III. Determination of current-strength in circuit corresponding to various intensities of luminous power in lamp and deflections of galvanometer.

With these data, the determination of relation of luminous power to energy expended in the lamp itself in producing the same was a matter of direct calculation.

I. *Determination of the resistance of the carbon-loop of lamp as compared with its luminous power and total heat developed.* —A preliminary experiment having shown that between 50 and 60 cells of a Grove battery, with active zinc surface of 20 square inches and platinum surface of 18 square inches in each cell, were required to develop the requisite electric current, such a battery was set up and connected piecemeal with the rest of the apparatus arranged as follows.

The battery-current was divided into two branches, which traversed in opposite directions the two equal coils of a differential galvanometer having ·33 ohm resistance in each coil. One branch then traversed the lamp, which was placed in a Bunsen photometer made by Sugg, of London. The other

branch passed through a series of adjustable resistances composed of German-silver wire, stretched in the free air of the laboratory to avoid heating (careful tests showed that this precaution fully accomplished the desired result); and the united branches were then carried to the other pole of the battery.

These arrangements having been made, a certain number of battery-cells were put in circuit and the resistances adjusted until the galvanometer showed no deflection. The condition of the loop was then observed in perfect darkness; and when its light was measurable it was taken by varying the distances of both lamp and candle as circumstances required.

Thus, for the lowest candle-power taken, the lamp was at 15·8 inches from the photometer, and the candle at 50 inches. The results so obtained were as follows:—

No. of cells in circuit.	Candle-power.	Resistance.
0	0	123·0 ohms.
5	0	113·5 "
10	dark red	106·0 "
20	·1 candle	94·0 "
25	·2 "	89·0 "
30	·4 "	87·0 "
35	·9 "	83·7 "
40	1·9 "	82·0 "
45	5·1 candles	79·8 "
50	8·4 "	78·0 "
58	18·0 "	75·0 "

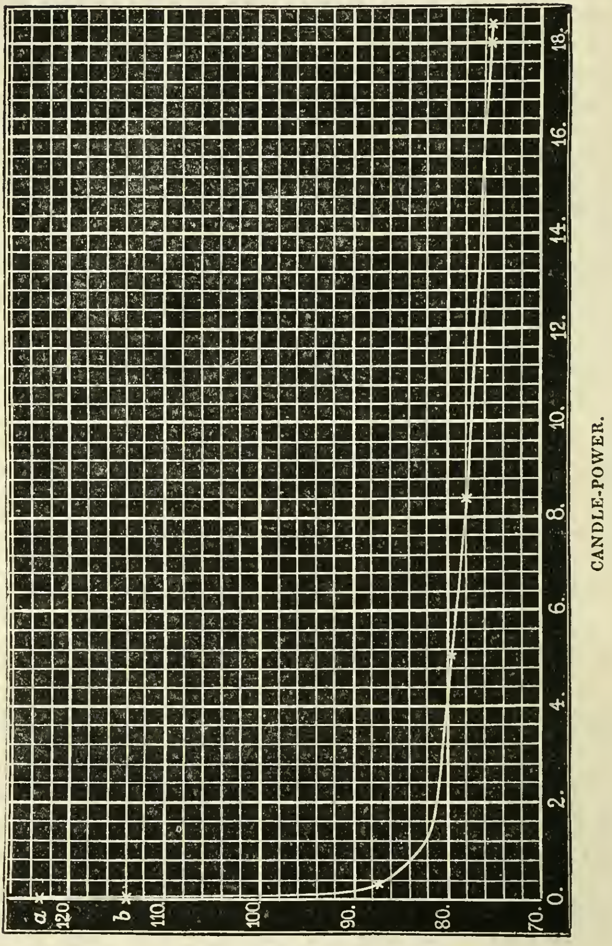
These results are also expressed in the curve shown in fig. 1.

The fact of a decrease of resistance with rise in temperature with carbon was previously noticed by Matthiessen in 1858 (see Phil. Mag. vol. xvi. pp. 220–221). This experimenter found the electric conductivity of ordinary gas-coke to rise about 12 per cent. between the common temperature and a light red heat.

In the case of this delicate thread of impure carbon constituting the loop of the lamp, the rate of increase in conductivity or fall in resistance is more rapid. Fig. 1 (p. 24) shows the above observations plotted as a curve, and needs no further explanation.

In the above discussion we have compared the resistance of the lamps with the luminous emissions only; but we have once considered it worth while to make an analogous but more extended comparison, namely one between the resistance and the total heat, or total heat and light, generated in the lamp. This enables us to carry the range of comparison below those points at which sensible light is developed. As a matter of course, this relation to total heat is also the relation to energy

Fig. 1.



RESISTANCE, IN OHMS.

transformed; and this also we have given in the following Table, expressed in foot-pounds and in horse-power.

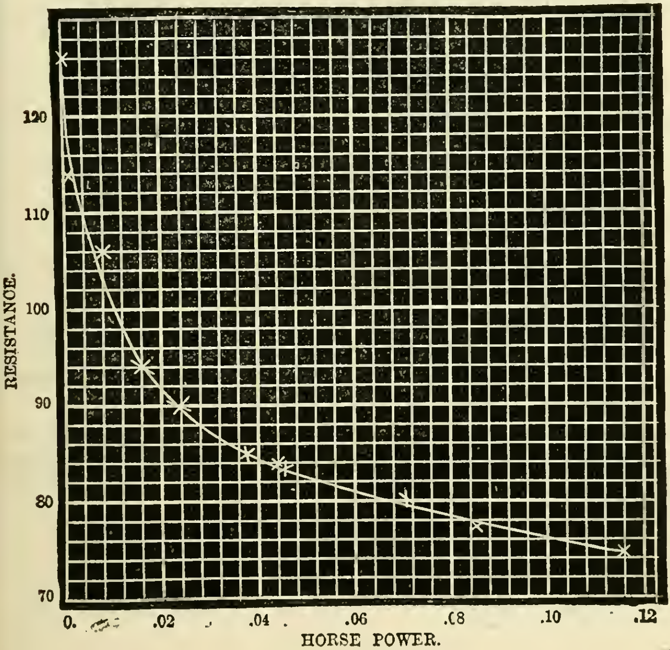
These results are also expressed in the curve (fig. 2); and it is interesting to notice the general similarity of this curve to that of fig. 1.

It might at first seem desirable to establish a temperature-ratio in the same connexion; but when we reflect that this would depend on a number of conditions liable to variation with individual lamps, and would really have no practical

bearing on the question of power consumed and light produced, it will be seen that this line of investigation hardly promised enough to warrant us in pursuing it. For example, if the carbon loop were surrounded by a less perfect vacuum, or by one or another gas such as nitrogen or hydrogen, there might be great differences in temperature even with the same resistance and current, or total heat.

Energy transformed into heat, and into heat and light in loop of lamp.			Resistance, in ohms.	Candle-power.
As foot-pounds.	As horse-power.	As total heat-units.		
66	·002	·0855	114	0
83	·0025	·1069	112	Just visible.
122	·004	·1710	111	Dull red.
244	·008	·342	106	Cherry-red.
488	·016	·684	94	·016
792	·024	1·026	90	·10
1254	·038	1·624	84·4	·59
1452	·044	1·881	84	·83
1518	·046	1·966	83·3	1·10
1650	·050	2·137	82·5	1·5
2343	·071	3·035	80	4·5
2838	·086	3·676	77·6	9·2
3828	·116	4·958	74·5	20·0

Fig. 2.



II. *Determination of the average light of the lamp in all azimuths.*—It was noticed at once that there was a vast difference between the amount of light given out by the lamp in a direction transverse to the plane of the loop and in the direction of that plane, the former quantity being about three times as great as the latter; and while it would of course be possible, on certain assumptions, to estimate what should be the average, it was also perceived that a direct determination by experiment would be far more reliable and important than any amount of theoretical discussion.

The lamp was therefore mounted on a divided circle with the axis of the lamp passing through its centre. A fixed index measured the angle of rotation of the circle and lamp, and was so placed as to read 0° when the plane of the loop was in the axis of the photometer. This was indicated by a well-defined line of shadow of the nearer half thrown by the further half of the loop on the photometer-disk.

The lamp was rotated 10° at a time; and several readings were made in each position, the averages of which are given in the accompanying Table.

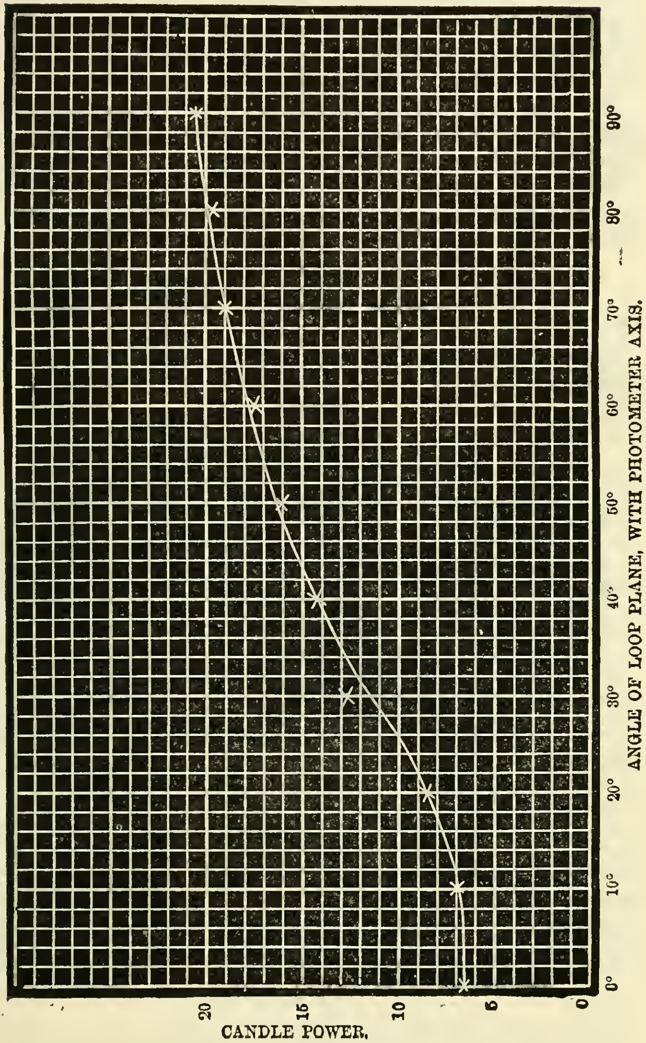
Candle-power of Loop in various Azimuths.

Angle of plane of loop to axis of photometer.	Candle-power.
0°	6·7
10	6·9
20	8·4
30	12·8
40	14·3
50	16·3
60	17·7
70	19·1
80	19·8
90	20·6
	10)142·6

Average = 14·26 = 69 per cent. of maximum.

This shows the results for one quadrant. Similar experiments were made for the three other quadrants, with like results. Fig. 3 exhibits the results of the Table plotted in a curve.

Fig. 3.


ANGLE OF LOOP PLANE, WITH PHOTOMETER AXIS.
0°
10°
20°
30°
40°
50°
60°
70°
80°
90°
CANDLE POWER.
20
15
10
5
0

III. *Determination of current-strength in circuit corresponding to various intensities of luminous power in lamp and of deflections of galvanometer.*—For these determinations the apparatus was arranged as follows:—The battery-current was passed through one coil of a Gaugain galvanometer, then through a copper voltameter, and then through the lamp placed in the photometer, thence returning to battery.

The amount of copper deposited in a known time gave of course the current-strength, in view of the fact that a current of one weber deposits ·326 milligramme of copper in a second.

Thus, in the first experiment, 1062·4 milligrammes were deposited in an hour, or in 3600 seconds; therefore

$$\frac{1062{\cdot}4}{{\cdot}326 \times 3600} = {\cdot}905 \text{ weber current.}$$

Three experiments were made of this sort, the data and results of which are given in the following Table:—

Weights of cathode.			Time.	Current, in webers.	Maximum candle-power.
Before.	After.	Gain.			
43398·4	44460·8	1062·4	60 min.	·905	15
48314·0	49110·0	796·0	40 „	1·017	17·6
43105·0	43617·0	512·0	25 „	1·047	19·8

From the current in webers and the corresponding resistance in ohms, where these had been determined as above, it was of course easy to deduce the exact amount of energy transformed into light and heat, and to compare the same with the actual candle-power afforded by the lamp at the same time*.

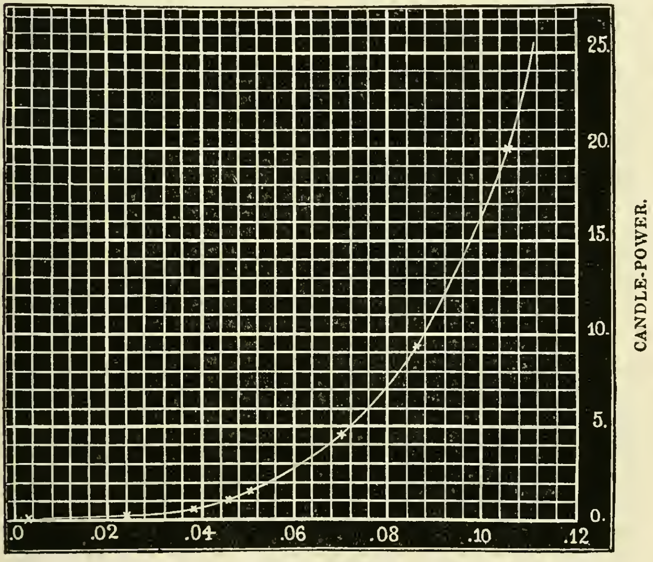
To make the results more general, however, the constant for the tangent-galvanometer used in all the experiments was determined so that the current-strength corresponding to its readings could be obtained in the cases where the voltameter had not been employed. This constant was found to be ·262; so that the tangent of the galvanometer-reading multiplied by ·262 gave the current-strengths in all cases.

* Thus, for example, in one experiment the average candle-power being 10+ candles, the resistance of the lamp was 76 ohms and the current ·905 weber.

IV. *Determination of power consumed by lamp only in maintaining light of different intensities.*

No. of cells of battery.	Deflection of galvanometer.	Current, in webers per second.	Resistance of lamp.	Horse-power.	Candle-power.	
					Maximum.	Average.
8	23·0°	·111	114·0	·002	0	
9	26·2	·129	112·0	·0025	Just visible.	
10	32·0	·164	111·0	·004	Dull red.	
15	42·4	·239	106·0	·008	Cherry-red.	
20	54·0	·361	94·0	·016	·016	
25	59·5	·445	90·0	·024	·10	·07
30	65·6	·578	84·4	·038	·59	·41
33	67·3	·626	84·0	·044	·83	·58
34	67·9	·645	83·3	·046	1·10	·77
35	68·7	·672	82·5	·050	1·5	1·05
40	72·1	·811	80·0	·071	4·5	3·1
45	73·9	·908	77·6	·086	9·2	6·4
50	76·3	1·079	74·5	·116	20·0	14·0

Fig. 4.



HORSE-POWER.

As regards the economic relations of this subject, it will be intereresting to notice that, an average light of 14 candles being obtained at the expense of 0·116 of a horse-power in electric current, each horse-power of electric energy would furnish 120 candle-power in these lamps. To obtain this horse-power of electric energy, however, considerably more

mechanical energy must be applied to the driving-pulley of the electric generator. If the loss so encountered was 40 per cent., as appears to be the case with some of the best machines which have been measured accurately, this would reduce the light developed to 72 candle-power for each horse-power of mechanical energy applied to the driving-pulley of the electric generator.

When we remember that with the arc-light there has been obtained from 1200 to 1800 candle-power per horse-power of mechanical energy applied to the generator, it is evident that Mr. Edison's lamp as now made does not escape the enormous loss which has heretofore been encountered by all forms of incandescent electric lamps.

IV. *On Electrical Expansion. By* Dr. G. Quincke, *Professor of Physics in the University of Heidelberg*.*

An extensive research on the action of electricity upon bodies which conduct badly has given the following results:—

1. Solid and liquid bodies suffer change of volume when they are exposed to the action of electric force in the same way as the glass of a Leyden jar.

2. This change of volume is not caused by change of temperature; generally it is an expansion; but with a few substances, for example with the fatty oils, contraction takes place.

3. No change was observed in the case of air exposed to electric force. If any change of volume occurs, it must be less than $\frac{1}{3000000000}$ of the original volume.

Alteration of Volume under the Influence of Electricity.

4. The electric expansion of glass is most conveniently observed with a common thermometer, according to the method employed by Fontana †, Govi ‡, and Duter §. The thermometer was placed in a metal vessel filled with melting ice; the liquid inside the thermometer (water, mercury, or saline solutions) formed the inner, the melting ice formed the outer coating of a Leyden jar.

When the two coatings of such a thermometer condenser are placed in communication with an electrical machine charged to a definite amount, or with a larger Leyden jar, the liquid in the tube of the thermometer falls. The level of the

* Translated from the *Berichte der königl. Akademie der Wissenschaften* of Berlin, of Feb. 19th, 1880.

† *Lettere inedite di Alessandro Volta* (Pesaro, 1834), p. 15.

‡ *N. Cim.* xxi.–xxii. p. 18 (1865–66). *C. R.* lxxxvii. 1878, p. 857.

§ *C. R.* lxxxvii. 1878, p. 828.

liquid in the tube was observed by the aid of a horizontal microscope provided with a micrometer-eyepiece; and the dimensions of the apparatus were so chosen that a change in volume of $\frac{1}{100000000}$ of the original volume could be observed.

The change of volume Δv is greater the larger the volume v of the bulb of the thermometer is. The change in volume follows instantaneously with flint-glass, more slowly with Thuringian glass (which is a better conductor of electricity). Upon discharge, the fluid returns nearly to its original position, immediately in the case of flint-glass, more slowly with Thuringian glass.

5. The residual displacement (in the same direction as the original displacement) after discharge is very small with flint-glass and larger with Thuringian glass, and appears to depend upon the electric polarization of the glass.

6. Under similar circumstances, the change of volume is greater only by an insignificant amount when the thermometer is filled with mercury instead of water.

7. The volume of the exterior of the thermometer-bulb increases simultaneously with the interior and to the same extent.

8. If the outer surface of the thermometer-bulb be covered with a thin layer of silver, the change of volume produced by electric force is the same, whether the bulb be surrounded by water or by air.

9. This change of volume is almost entirely independent of hydrostatic pressure of the fluid on the walls of the bulb.

10. With the same apparatus, under apparently identical conditions, the change of volume is sometimes greater and sometimes less, according as the glass has remained a longer or a shorter time without being charged with electricity.

11. The expansion $\frac{\Delta v}{v}$ is nearly, but not exactly, proportional to the square of the difference in electric potential of the two coatings of the thermometer-bulb, and inversely proportional to the square of the thickness of the walls of the thermometer-bulb.

With English flint-glass and a thickness of glass of from 0·142 to to 0·591 millim., the change in volume varied from 10·67 to 0·19 millionths of the original volume, with a spark-distance of 2 millims. between spheres of brass of 20 millims. diameter. With an equal spark-distance, the change in volume for Thuringian glass varied from 4·61 to 0·36 millionths of the original volume with a thickness of glass from 0·238 to 0·700 millim.

With greater change of volume than 10 to 12 millionths of

the original volume, the glass was generally perforated and the apparatus broken. A change of volume of 68 millionths without fracture was observed only with a particular kind of German glass.

12. Similar changes of volume were observed upon electrifying thermometer condensers of mica, quartz, and caoutchouc filled with water.

The changes of volume observed with many kinds of mica, and with caoutchouc which had remained in contact with water for a long time, were comparable with those observed with glass; but with fresh caoutchouc the change is about ten times as much.

13. In experiments with vessels of caoutchouc there is observed at the same time a percolation of water through the pores of the caoutchouc under the influence of the electricity.

Alteration of Length under the Influence of Electricity.

14. Narrow glass tubes 1000 to 1200 millims. long were coated inside and out with silver; the two coatings were insulated from each other and connected with the coatings of a charged Leyden jar. On charging such a tube condenser, expansion took place, which disappeared again almost entirely upon discharge.

The expansion was measured best with Oertling's contact-lever, by means of which an expansion of 0·004 millim. could be measured directly, and a tenth of this amount could be estimated with certainty.

The expansion Δl of glass rods under electric influence, already investigated by Righi*, takes places essentially according to the same laws as the change in volume of the thermometer condensers. Its magnitude increases with the length l of the rod.

The electric expansion $\frac{\Delta l}{l}$ is nearly, but not exactly, proportional to the square of the difference in electric potential of the coatings of the tube condenser, and inversely proportional to the square of the thickness of the glass. With a difference of potential corresponding to a spark-distance of 2 millims. between brass spheres of 20 millims. diameter, the electric expansion of tubes of English flint-glass varied from 2·26 to 0·72 millionths of the original length for the thickness of glass of 0·097 to 0·186 millim.

15. Under similar circumstances the electric expansion was the same, and attained its maximum in the same time, whether the glass tubes were surrounded with air or water.

* *Comptes Rendus*, lxxxviii. 1879, p. 1263.

16. With the same difference of electric potential,

the increase in volume $\frac{\Delta v}{v}$ is,

for the same kind of glass and the same thickness,

three times the increase of length $\frac{\Delta l}{l}$.

17. The same result was obtained by simultaneous measurement of the increase in volume and the increase in length of a thermometer condenser with a long cylindrical bulb. The bulbs of the thermometer condensers were very uniform tubes of flint-glass, 800 to 1900 millims. in length and 0·362 to 0·621 millim. in the thickness of the glass. The bubble of the spirit-level of the contact-lever was observed with a microscope and micrometer-eyepiece, which permitted the measurement of an increase in length of 0·000008 millim.

18. The relationship $\frac{\Delta v}{v}=3\frac{\Delta l}{l}$ is not consistent with the hypothesis that the thickness of the glass is diminished by the attraction of the unlike electricities on the coatings of the condenser, and that the volume of the bulb of the thermometer is thus increased indirectly by "electric compression."

19. Electric expansion takes place equally in all directions, in the same way as thermic expansion. The hypothesis that electric expansion results from heat caused by feeble electric currents traversing the glass between the two coatings is negatived by the facts stated under 6, 8, and 15.

20. The electric expansion of glass is most simply shown by the use of tubes with excentric bore, in which the glass is thicker on the one side than on the other. Such a tube is curved, after cooling, with the thinner wall on the convex side, since the thicker wall cools more slowly and contracts more forcibly than the thinner wall. Such a glass thread, closed at the lower end, filled with water, and immersed in a deep vessel of water, forms a "glass-thread electrometer," of which the water within and without the tube serves for the coatings.

When these coatings are placed in contact with the coatings of a charged Leyden jar, the thread curves still more, since the thin wall is more expanded by the electric force than the thick wall. The deflection of the glass-thread electrometer may amount to several millimetres, and may be conveniently observed by means of a horizontal microscope with micrometer eyepiece.

Upon discharge, the end of the bent glass thread returns to its original position immediately with flint-glass, more slowly

with Thuringian glass. There remains, as with the changes of volume of the thermometer condenser, a residual displacement in the same direction as the original displacement, which gradually disappears.

21. With the same glass-thread electrometer, the displacement is proportional to the square of the difference of electric potential of the two coatings.

22. The residual displacement increases with the difference of electric potential and with the conductivity of glass; it seems therefore to depend, like the residual expansion of the the thermometer condenser, on the polarization of the glass.

23. By means of a suitable key the coatings of the thermometer condenser could be put into communication, first with the poles of a bichromate battery of 44 cells, and then with a sensitive mirror-galvanometer. The deflection of the galvanometer was then proportional to the electric capacity of the thermometer condenser. When the temperature rises, the change of volume corresponding to a given difference of potential is proportional to the increase of electric capacity for the same thermometer condenser.

24. Similarly, the deflections of a glass-thread electrometer, made of the same kind of glass as a thermometer condenser, increase in the same proportion as the electric capacity of the condenser. A temperature-increase of 1° C. corresponds to an increase of deflection or of capacity of about 0·003 of the original value for flint-glass and about 0·012 for Thuringian glass.

25. The greater the difference of electric potential and the higher the temperature of the glass, the more quickly do the deflections of a glass-thread electrometer take place. Flint-glass acts more slowly than Thuringian glass.

Changes of Elasticity caused by Electricity.

26. The elasticity of flint-glass, Thuringian glass, and caoutchouc is diminished by electricity; but that of mica and gutta-percha is increased.

27. A magnetic bar was hung at the lower end of a hollow glass thread silvered inside and outside, so that the magnetic axis was nearly at right angles to the magnetic meridian. The moment of the magnetic force was then equal and opposite to the moment of torsion of the glass thread corresponding to the angle ϕ through which it had been twisted.

A vertical mirror was attached to the magnetic bar, whose position was observed with a telescope and scale. When the coatings of the glass thread are connected with the coatings of a charged Leyden jar, the angle of torsion ϕ becomes greater by the amount $\Delta\phi$, while the magnetic moment re-

mains almost unaltered. The increase of the angle ϕ corresponds, therefore, to the decrease of the force of torsion of the suspension-thread; and $\frac{\Delta\phi}{\phi}$ is the measure of the change of elasticity of the thread.

Upon discharge, the magnet and thread resume their original position.

The decrease in the force of torsion is nearly proportional to the square of the difference in electric potential of the two coatings, and is greater the less the thickness of the walls of the glass thread.

With a quantity of electricity 20 in the Leyden jar used (of six jars), and a thickness of glass of the suspension-thread 0·1 millim., $\frac{\Delta\phi}{\phi}$ was 0·00055 for flint-glass and 0·002 for Thuringian glass.

An indiarubber tube, gilded on the outside and filled with water, showed with the same Leyden jar about the same change $\frac{\Delta\phi}{\phi}$ as the far thinner thread of Thuringian glass.

28. A strip of mica 840 millims. long, 30 millims. broad, and 0·04 millim. thick, covered with gold leaf on one side and carrying a magnetic bar, in the same way gave an increase of torsion of $\frac{1}{13}$ of the original value when the coated side was connected with the earth, and the uncoated side electrified by means of a metallic comb connected with a Holtz's machine which was passed along it. A band of gutta percha of like dimensions showed under the same circumstances an increase in torsion of 0·00316 of the original value.

29. Similar results were obtained in experiments in which the torsion of the glass thread was balanced by the torsion of a metallic wire.

Electric Expansion of Fluids.

30. The expansion produced by electric force may be observed with liquids as well as with solids by using a voltameter with platinum electrodes filled with the liquid, the delivery tube of which is replaced by a vertical capillary tube. The apparatus is maintained at a constant temperature by means of melting snow. An increase of volume is observed when the platinum electrodes are connected with the coatings of a charged Leyden jar. When the battery is discharged the fluid returns to its original position in the capillary tube.

With fluids which are good insulators, such as sulphide of carbon and ethereal oils, the Leyden battery retains its charge

for some time, and the increase of volume persists for the same length of time; it then disappears gradually. With better conductors (as glycerine, alcohol, and water) the level rises almost instantly, and the battery is discharged immediately.

The same quantity of negative or positive electricity in the Leyden battery gives nearly the same change of volume of the liquid. The change of volume is nearly proportional to $\frac{q^2}{s}$ when q denotes the quantity of electricity and s the charged surface of the Leyden jar.

31. With fluids which insulate well, the electrodes may be connected with a Holtz's machine instead of a Leyden battery.

32. With fatty oils which insulate well, a decrease of volume takes place instead of an increase.

33. When the electric charge is too great, a spark passes in the fluid between the platinum electrodes, and the voltameter is broken. Hence the different liquids could not be examined in the same apparatus.

The following table gives an approximate comparison of electric dilatation with thermic dilatation, the expansion being multiplied by 1,000,000. The electric expansion is that of a layer of substance about 12 millims. in thickness, acted on by the electricity.

$$\text{Expansion } \frac{\Delta v}{v} \times 10^6.$$

	By increase of temperature from 0°C. to 1°C.	By a quantity of electricity.	
		±20	±40
Carbon disulphide	1141	5·23	22·43
Alcohol	1042	6·80	35·50
Petroleum	1017	5·66	......
Turpentine	902	1·70	42·45
Glycerine	512	0·59	3·19
Distilled water at 8° C.	92	0·07	0·23
Water with a trace of hydrochloric acid at 10° C.	......	0·13	0·42
Water with 0·124 % hydrochloric acid at 13°	......	0·07	0·56
Distilled water at 0°	−20	−0·03	−0·09
Water at 0° with a trace of hydrochloric acid	......	−0·06	−0·30
Water with 0·124 % hydrochloric acid at 0°	......	−0·03	−0·36
Thuringian glass	32	0·003	0·010
Flint glass	26	0·002	0·009
Colza oil	773	−18·24	......
Almond oil	775	−6·85	......

It is remarkable that the order of the substances examined, whether they be arranged according to thermic expansion or electric expansion, has no relation to the order of their conductivities for electricity.

34. Changes of temperature of a few hundredths of a degree suffice to bring about the change of volume produced by electricity in the first group of substances, that to which water belongs.

The length of time that the expansion lasts in the case of insulating fluids is opposed to the hypothesis of an indirect expansion, resulting from heat produced by feeble currents of electricity between the electrodes—as is also the small increase of the expansion when the conducting-power of water is increased by the addition of hydrochloric acid, and also the *decrease* in volume in the case of the fatty oils, which also expand upon increase of temperature from 0° C.

Electric Perforation of Glass.

35. Electric force produces generally the same effect as heat, namely expansion. But as by unequal application of heat to different portions of a body it may be broken, so also its fracture may be brought about by the unequal action of electric force.

Uniform expansion by heat or by electric force is not capable of fracturing glass; but unequal thermic or electric expansion does so, and the more readily the greater the elastic tension produced in the interior of the glass.

What is true of glass is true of other substances. Thick masses, and such as conduct heat or electricity badly, are fractured more easily than thin masses, or such as conduct heat or electricity readily. This is confirmed by experiment.

Electric Double Refraction.

36. It is well known that by unequal application of heat solid transparent substances are unequally expanded, and become optically double-refracting.

In the same way, by unequal electric expansion, substances become optically double-refracting. This explains the double refraction observed by Kerr *, which glass, quartz, resin, and insulating fluids exhibit under the influence of electricity, and the apparent contradiction of these results with those obtained by other observers.

If long thin glass plates are coated with tinfoil and power-

* Phil. Mag. (4) l. pp. 337–348, 446–558, 1875; ib. (5) viii. pp. 85–102, 229–245, 1879.

fully electrified, like a Franklin's plate, there is no double refraction, as was known to me from former experiments, and as also Gordon * and Mackenzie † have found.

The glass is exposed to nearly equal electric force at all points, and uniformly expanded; it is therefore no more doubly refracting than equally heated glass.

But if one of the tinfoil coatings be replaced by mercury in a glass tube of 30 millims. exterior diameter and 14 millims. inner diameter, the end of which has been ground and carefully cemented to the glass plate so that only the glass under the mercury is electrically expanded, the glass is then subjected to unequal electric action, and becomes doubly refracting.

37. If a fluid has heat communicated to particular portions by means of a hot metal plunged in it more quickly than can be conveyed away by conduction and convection, it becomes optically doubly refractive, in the same way as unequally heated glass.

In the same way, a fluid between two metallic electrodes becomes doubly refractive when these are maintained at unequal electric potential. The unequal electric expansion is dependent upon the velocity with which the electricity diffuses itself in the fluid and produces the electric expansion. The expansion is nearly proportional to the square of the electric force active at a given point of the insulator. The expansion must therefore be the greatest along the shortest line of electric force between the electrodes.

Substances whose refractive index increases with increase of temperature, such as glass, and those whose refractive index decrease, such as sulphide of carbon, will behave differently if they are both expanded by electricity and if thermic and electric expansion alter optical properties in the same direction.

In fact, glass and sulphide of carbon show opposed electric double refraction according to the observations of Kerr—a result which my experiments have confirmed.

Further, when substances (such as sulphide of carbon and colza oil) are oppositely affected by electric force, both have their refractive index diminished by thermic expansion; then, again, they must show opposed electric double refraction.

This conclusion also is confirmed by experiment.

38. If a piece of tinfoil be heated by an electric current while between two homogeneous glass cubes, the glass becomes doubly refractive, as if it expanded at right angles to the tin-

* Phil. Mag. (5) ii. p. 203, 1876. † Wiedem. *Ann.* ii. p. 356, 1877.

foil, and contracted parallel to it. The glass behaves like a negative crystal * (Iceland spar) with optic axis parallel to the warmed plate.

The line of greatest heating parallel to the tinfoil or the optic axis of a negative crystal must correspond, in the case of electric double refraction, to the shortest line of electric force between the metallic electrodes, as, in fact, Dr. Kerr † has asserted, and as I have found confirmed.

Moreover those portions of insulators in the neighbourhood of the shortest line of electric force for those substances which Dr. Kerr calls negative (glass, fatty oils, &c.), behave like optically negative crystals with optic axis parallel to the shortest line of electric force; "positive" substances (sulphide of carbon, &c.) behave like optically positive crystals with the optic axis parallel to the shortest line of electric force.

More precisely, solid and fluid insulators between metallic electrodes must be regarded as unequally expanded bodies, which act optically like an aggregate of numerous small crystals. The resin called by Dr. Kerr "clear amber resin" behaves oppositely to glass when subjected to electric double refraction, and will probably decrease in volume like the fatty oils under the influence of electric force.

39. The optical phenomena observed with substances which conduct electricity badly, completely confirm the production of expansion and contraction by electric force observed in other ways.

40. The cause of electric expansion and of changes of elasticity produced by electric force is to be sought in a twisting and displacement of the molecules of the insulator—which place themselves with their greatest length in the direction of the resultant electric force, so that their electric moment is a maximum. That small particles of glass and other insulators, suspended in badly-conducting fluids, do actually take up such a position, has been shown by Weyl ‡. If the particles are scattered not in a fluid, but in a mass not perfectly solid, similar changes must take place, but more slowly.

* Compare F. E. Neumann, *Abh. Berl. Ak.* 1841, ii. p. 6.
† Phil. Mag. (4) l. p. 337, 1875.
‡ Reichert and Du Bois's *Arch.* 1876, p. 721.

V. *On the Criterion by which the Critical Point of a Gas may be determined.* *By* J. DOUGLAS HAMILTON DICKSON, *M.A., F.R.S.E., Tutor of St. Peter's College, Cambridge**.

IT is generally assumed that at the critical point of a gas the internal latent heat, λ, vanishes. From the known equation for the internal latent heat,

$$\lambda = \frac{1}{J}(u-v)\left(T\frac{dp}{dt}-p\right),$$

we get the condition $T\frac{dp}{dt}-p=0$. In order to support this, Avenarius has made experiments on ether to show that u is *not* equal to v at the critical point.

On the assumption, however, of the correctness of the formula given by Clausius in the last Number of this Magazine (June 1880, p. 401), viz.

$$p = \frac{RT}{v-\alpha} - \frac{c}{T(v+\beta)^2}, \quad . \quad . \quad . \quad . \quad (1)$$

where, for carbonic acid,

$$\left.\begin{aligned} R &= \cdot 003688, \\ c &= 2\cdot 0935, \\ \alpha &= \cdot 000843, \\ \beta &= \cdot 000977, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the pressure being reckoned in atmospheres, and the unit volume being that of carbonic acid at one atmosphere at the freezing-point; the critical point may be determined by assuming James Thomson's *form* of an isothermal below the critical point, and in the cubic (1) for v (p and T being constant), putting v_1 (the volume of the liquid) equal to v_3 (the volume of the gas)—and therefore, by the Theory of Equations, *making its roots equal.* This is in opposition to what Avenarius wished to establish by his experiments on ether.

Rewriting (1) as a cubic for v, it becomes

$$v^3 - \left(\frac{RT}{p} - 2\beta + \alpha\right)v^2 + \left(\frac{c}{pT} - 2\beta\frac{RT}{p} - 2\alpha\beta + \beta^2\right)v$$
$$- \left(\alpha\frac{c}{pT} + \beta^2\frac{RT}{p} + \alpha\beta^2\right) = 0. \quad . \quad . \quad (3)$$

The conditions for the roots of this being equal may be written

* Communicated by the Author.

$$\frac{RT}{p} - 2\beta + \alpha = 3\phi,$$

$$\frac{c}{pT} - 2\beta\frac{RT}{p} - 2\alpha\beta + \beta^2 = 3\phi^2,$$

$$\alpha\frac{c}{pT} + \beta^2\frac{RT}{p} + \alpha\beta^2 = \phi^3,$$

giving, for ϕ, the cubic

$$\phi^3 - 3\alpha\phi^2 - 3\beta(2\alpha+\beta)\phi - \beta^2(3\alpha+2\beta) = 0,$$

whose roots are $3\alpha+2\beta$, $-\beta$, $-\beta$. Of these, the first alone is admissible, from which

$$\frac{RT}{p} = 8(\alpha+\beta), \text{ and } \frac{c}{pT} = 27(\alpha+\beta)^2,$$

whence the temperature and pressure of the critical point are given by

$$\left.\begin{array}{l} T^2 = \dfrac{8}{27}\dfrac{c}{R(\alpha+\beta)}, \quad p^2 = \dfrac{1}{216}\dfrac{Rc}{(\alpha+\beta)^3}; \\ \text{and the volume at the critical point is } \phi, \textit{ i.e. } 3\alpha+2\beta. \end{array}\right\} \quad (4)$$

The substitution of Clausius's numbers (2) in equations (4) give,

$$\left.\begin{array}{r} T - 274 = 30^\circ, \\ p = 77, \\ v = \frac{1}{223}. \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

From Andrews's table for carbonic acid at 31°·1 (the nearest recorded to the critical temperature), the following table indicates the values of $\dfrac{-\Delta p}{\Delta v}$ between successive values of p:—

Table.—Temp. 31°·1.

p............	77·64	75·40	73·83	73·26	71·25	1
v............	51·3	60·9	108·7	112·2	128·2	18950
$-\Delta p \div \Delta v$...	·233	·033	·163	·126		

where the last column is *calculated* from the same table. At this temperature the least change of pressure for a finite change of volume takes place between the pressures 73·83 and 75·40—that is (say), at the pressure $74\frac{1}{2}$; and between these pressures

the mean of the volumes is $\frac{1}{223 \cdot 5}$. These results agree very remarkably with those (5) calculated from Clausius.

Reconsidering the question theoretically, and in the light of the curves of Andrews and James Thomson, we are led to the following criterion for the critical point—viz. from equation (1) at T, = const., we must have $\frac{d^2p}{dv^2}=0$ (*i. e.* a point of inflection), and $\frac{dp}{dv}=0$ (*i. e.* the tangent parallel to the axis of v). These give much more easily than before, and at once, the values

$$\left.\begin{aligned} v&=3\alpha+2\beta, \\ T^2&=\frac{8}{27}\frac{c}{R(\alpha+\beta)}, \\ p^2&=\frac{1}{216}\frac{Rc}{(\alpha+\beta)^3}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (4')$$

In short, provided the true theoretical relation between p, v, and t for a gas has been found, the critical point may be determined by equating the volume of the liquid to that of the gas, both being at the same temperature and pressure.

The above numerical results have been deduced from Clausius's formula for carbonic acid, verified by Andrews's table. The formula which is nearest to that of Clausius is given by Van der Waals (see Phil. Mag. June 1880, p. 398), and is

$$p=\frac{RT}{v-b}-\frac{a}{v^2};$$

where

$$R=\frac{1 \cdot 00646}{T_0(=273^\circ)}$$

$$a=\cdot 00874,$$

$$b=\cdot 0023.$$

Applying the theory to this equation, we get $T-273^\circ=29^\circ \cdot 25$, $p=61 \cdot 2$, and $v=\frac{1}{145}$; the discrepancy between these two last results and Andrews's observed ones being very considerable.

With regard to the position of the horizontal line (p constant) for a given isothermal, Clausius has assigned as the criterion by which to determine it, that, the straight line joined by the curved portion of the isothermal between its ends, constitutes a reversible cycle, which is such that the whole work done in it is zero. If ϖ be the unknown pressure along this straight line, and v_1, v_3 the least and greatest roots of the cubic

$$v^3-\left(\frac{\mathrm{RT}}{\varpi}-2\beta+\alpha\right)v^2+\left(\frac{c}{\varpi\mathrm{T}}-2\beta\frac{\mathrm{RT}}{\varpi}-2\alpha\beta+\beta^2\right)v$$
$$-\left(\alpha\frac{c}{\varpi\mathrm{T}}+\beta^2\frac{\mathrm{RT}}{\varpi}+\alpha\beta^2\right)=0, \quad . \quad (6)$$

the work equated to zero will give

$$\varpi=\frac{\mathrm{RT}}{v_3-v_1}\log_e\frac{v_3-\alpha}{v_1-\alpha}-\frac{c}{\mathrm{T}(v_1+\beta)(v_3+\beta)}. \quad . \quad . \quad (7)$$

These are two simultaneous equations for the roots of the cubic and ϖ, which can only be solved by trial and error. Guided by Andrews's table for CO_2 at $13^\circ\cdot1$, a temperature safely below the critical point, I have substituted for ϖ in equation (6) the values 49 and 50, obtaining respectively the cubics ($\phi=1000v$):—

$$\phi^3-20\cdot4\phi^2+106\phi-147=0, \quad . \quad . \quad . \quad . \quad (8)$$

whose roots are approximately,

$$v=\cdot002263, \quad \cdot004914, \quad \cdot013223;$$

and

$$\phi^3-20\phi^2+104\phi-144=0, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

whose roots are approximately,

$$v=\cdot002241, \quad \cdot00506, \quad \cdot01271.$$

From these values of v_1 and v_3 substituted in equation (7), the pressures obtained are respectively 49·50 and 49·60. The value 49·5 for ϖ was then tried in equation (6), and gave, from equation (7), the pressure 49·3.

These calculations show that Clausius's formula represents, with as great accuracy as we can obtain at present, the whole of the experimental results given by Andrews for carbonic acid. It is highly probable that a similar formula will be applicable to other substances: but before regarding it as a general formula, equally accurate experiments must be made upon other gases. I intend to examine the results recently given by Ansdell, in order to test further the applicability of the general formula.

VI. *On the Determination of the Acceleration of Gravity for Tokio, Japan.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOUR correspondent is, we presume, the Major Herschel who recently in 'Nature' commenced his review on Colonel Clarke's 'Geodesy' thus:—"It is well that there are men brave enough with the pen as there are others brave with

the surgeon's knife, or the soldier's bayonet." This is true, provided the knife and the bayonet are handled with skill and for the benefit of mankind. But does not Major Herschel's review, like his present criticism of our paper, instinctively suggest rather the hewing of the professional fighting man than the courage of the skilful surgeon.

I. To his first objection, that our doctrine regarding laboratory teaching is "as erroneous as the design is laudable," we would reply that as our paper was read at the Physical Society, of which many of the Members are teachers of physics, and anxious, like ourselves, to ascertain the best mode of carrying on experimental work in a physical laboratory containing many young students, we considered it not inopportune to mention that some years' practice in laboratory teaching had led us to strongly advocate the plan of students assisting in original investigations, and to deprecate their merely repeating well-known lecture-experiments, very commonly the whole laboratory work a student is set to do. Our concluding remarks were not, as Major Herschel seems to think, directed against practice in the use either of Kater's reversible pendulum, or, in fact, of any other standard measuring-instrument.

II. Your correspondent charges us with "neglect of the commonest and most essential principles of exact experimentation." Now, as the Editors of the Philosophical Magazine* have allowed this grave charge, one of the gravest that could be brought against scientific men, to be made in the pages of their Journal, it demands careful consideration. To prove his accusation and to show that he is "not making a random charge," Major Herschel quotes a paragraph from our paper in which we explain that the well-known "corrections for infinitely small arcs and for the air-friction" are negligible in the case of our long pendulum, "on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible even after many swings," and in which we further mention that, fearing the possibility of an "error arising from its flexibility and slight elasticity which would not affect a rigid compound pendulum," we endeavoured to calculate the magnitude of the special possible errors for a long wire pendulum. This Major Herschel frankly states perhaps he

* [I regret much the appearance of Mr. Herschel's letter, not so much because I believe the writer to be, in the main, wrong on the scientific points discussed in it, as because I do not consider its tone and temper suitable for publication in the Philosophical Magazine. I feel confident that my brother Editors agree with me in regretting for the last-mentioned reason the publication of the Letter, which took place through an oversight in this respect.—W. Thomson, Ed. *Phil. Mag.*]

ought to regard "as dust thrown into the pupils' eyes, to prevent their attending to such well-known reductions as those for arc, buoyancy, resistance, &c." It is not quite clear from this whether (a) he means that these corrections were not made in the experiments with the Kater's pendulums about which he goes on to speak; or whether he means (b) that they were not made in the experiments with our long wire pendulum, from which alone our value of g was determined; or whether, lastly, he desires to imply (c) that, being unacquainted with the method of applying such simple corrections, and being of course also unacquainted with any text-book in which an account of such corrections could be found, we slurred over the matter and intentionally abstracted the students' attention by "abstruse investigations of conditions which, almost obviously, are unimportant."

(a) We reply that, as we do not give in our paper, or in any way use for the numbers there mentioned, any of the experimental results obtained with the Kater's pendulum, it appeared to us unnecessary to describe the corrections proper to be employed when using the reversible pendulum. It may, however, now be mentioned that, after the knife-edges of the reversible pendulum had been finally adjusted so that the pendulum beat seconds either end up, it was always found that the distance between them was slightly greater than one metre. Now the reductions for arc, for buoyancy, and for resistance would, when applied, have each separately slightly increased this apparent length of the seconds-pendulum. Consequently, as on no part of the earth's surface is the length of the equivalent simple seconds-pendulum greater than one metre, it follows that, whatever may have been the cause for our results with the reversible pendulums not being satisfactory, whether it was due to our own inexperience, or whether it arose from defective workmanship in the instrument itself, want of absolute parallelism of the knife-edges, &c., it certainly could not have been due, as Major Herschel appears to imply, to any want of "attending to such well-known reductions as those for arc, buoyancy, resistance, &c."

(b) and (c). But even were this not so, would Major Herschel be justified in assuming that we made no corrections when using the Kater's pendulums, seeing that we have made and given in our paper the results of the much smaller corrections for arc, for buoyancy, and for resistance in the case of the long pendulum, in addition to those other corrections which your correspondent is pleased to call "abstruse and almost obviously unimportant," but which have appeared to Sir William Thomson of sufficient interest to induce him,

since the reading of our paper, to take up the complete mathematical investigation of the influence of the "kick" we referred to on the time of vibration of a long wire pendulum.

First, for Arc.—We said that the result of this correction was unimportant, and we did so for this reason. The length of the pendulum was 939·09 centims.; the amplitude or the length of a complete swing backwards and forwards varied on different days from 50 to 20 centims., but was always less than 30 centims. for the observations given in our paper; so that we have to consider an elongation of $7\frac{1}{2}$ centims. on each side of the vertical line. The correcting factor to reduce to infinitely small arcs is, of course,

$$\left\{1+\frac{1}{16}\left(\frac{7·5}{939·09}\right)^2\right\}^2,$$

which leads to an addition of only $\frac{1}{125000}$ of the whole value for g, considerably less in fact than the tenth of a millimetre per second per second.

Secondly, for Buoyancy.—On page 300 of our paper we said this correction adds 0·16 centim. per second per second to the value of g, which calculation may easily be verified from the given size of the brass ball, namely 8·20 centims. in diameter.

Thirdly, for Resistance.—We stated that this was negligible; and we still do not see how it can be otherwise when the resistances due to air-friction, to the tip of the platinum wire at the bottom of the bob passing at each swing through the two or three millimetres of mercury, &c. were so small that the well-known formula for n swings,

$$\lambda=\frac{1}{n-1}\log_\epsilon\left(\frac{c_1}{c_n}\right),$$

failed to give, even for a considerable value of n, a value of this logarithmic decrement differing sensibly from nought. In fact it is well known that, for a metal ball of 8 or more centimetres diameter, the resistance arising from viscous friction of the air is insignificant.

The number then for g, as given in our paper, was corrected for arc, for buoyancy, and for resistance.

There is, however, a fourth correction, which, although sometimes included under "resistance," is not a correction for a resistance or retarding force at all, but for the increased mass moved due to the inertia of the air—a correction which, by the by, can only be applied in the roughest way possible to a Kater's pendulum, or to any pendulum other than a sphere suspended by an infinitely thin wire. This air-inertia correction, which is perfectly definite for a simple pendulum such as we

were using, was by accident omitted in our final calculation; and we are glad that Major Herschel's having charged us with neglecting the corrections for arc, for buoyancy, and for resistance, all of which we had attended to, has enabled us to add another correction which had been omitted. Applying this latter to the data we have given, it is easily seen that our experiments lead to a value of g in Tokio equal to 979·82 centims. per second per second.

Not only is this correction extremely difficult to apply, with any degree of accuracy, to a Kater's pendulum, but it is different for the two axes of suspension. It would, however, like all the other corrections, be of a nature to make the real value of g greater than the apparent; so that its employment or non-employment will not explain why our results with the reversible pendulums were unsatisfactory.

To avoid having to apply this air-inertia correction, it is of course not unusual to swing the Kater's pendulum in a vacuum; but even then the correction for viscosity will not be diminished, since it has been shown by the late Prof. Clerk Maxwell and by Mr. Crookes that it is not until the very high vacua of a good Sprengel pump are reached that the viscosity of the air is sensibly diminished. The corrections, then, necessary to apply to a reversible pendulum swinging in an ordinary vacuum will be more numerous than in the case of a long wire pendulum like ours swinging in the air, not to mention the far greater facility with which experiments can be made with the latter kind of pendulum.

III. We beg to thank Major Herschel for drawing our attention to a misprint of λ for 2λ in the formula for calculating g given by us. This misprint, however, which escaped our notice when correcting the proof, is of but little consequence, seeing that it was obviously the correct formula we employed in the calculation itself; for the formula as it stands would lead to the value only 978·57 for the latitude 35° 39′. But your correspondent says the formula itself is "wrong numerically," meaning, of course, that the numerical coefficients are erroneous. These coefficients, however, are, figure for figure, those given by Dr. Everett on page 21 of his 'Units and Physical Constants,' published last year. Now, although the latest pendulum-experiments make it probable that the numerical coefficients mentioned by Major Herschel are still more accurate than those given in the book we have quoted, he is a little unfortunate in taking this occasion to object to Dr. Everett's formula, seeing that the latitude of Tokio is just that which gives to $\cos 2\lambda$ such a value as to make the two formulæ yield identical results, or at least results differing by

less than the tenth of a millimetre per second per second—that is, by less than the one hundred thousandth part of the value of g; so that, seeing that local circumstances would probably make the real value of g differ by more than the one hundred thousandth part from the calculated value, Major Herschel's correction is of absolutely no importance for the object of our paper, which concerned the value of g in Tokio.

But Major Herschel says, in disparagement of our experimental determination of the value of g for Tokio, Japan, that the formula "no more belongs to Clairaut than to Copernicus." In the table of contents, however, of Thomson and Tait's standard work on Natural Philosophy we find "Clairault's formula for the amount of Gravity;" and in section 222, page 167, there referred to, it is stated:—

"The formula deduced by Clairault from observation, and a certain theory regarding the figure and density of the earth may be employed to calculate the most probable value of the apparent force of gravity. This formula, with the two coefficients which it involves, corrected according to the best pendulum observations (Airy, Encyc. Metr., Figure of the Earth) is as follows:—

"Let G be the apparent force of gravity on a unit mass at the equator, and g that at any latitute λ; then

$$g = G\,(1 + \cdot 005133 \sin^2 \lambda)."$$

If, then, Clairault proposed any formula of the form

$$a + b \sin^2 \lambda,$$

it will still, it appears to us, be Clairault's formula when put in the form

$$(a + \frac{b}{2}) - \frac{b}{2} \cos 2\lambda;$$

nor do we think that the unit of length, English, French, Japanese, &c., which may be involved in the expression for the acceleration due to gravity at different latitudes affects the name of the formula.

That our having compared the value of g obtained from a long course of experiments, with the *approximate* value calculated from the standard formula, should have caused Major Herschel to be unable to "find words to represent adequately the state of dazed astonishment created by such an appeal," is to be regretted, but, we are afraid, cannot be helped, seeing that the comparison of results obtained experimentally with those previously calculated from theoretical considerations is the ordinary method of procedure in physical investigations; and, indeed, we may add that in Col. Clarke's 'Geodesy' the great

use made of all trustworthy pendulum-observation is to determine the constants of Clairault's formula.

IV. Major Herschel criticises the statement in our paper, "Captain Clarke has found that the equator is elliptical," says that it is wrong, and that it should be :—"What he [Captain Clarke] has taught is in effect this: *Assuming an equator more or less elliptical, the ellipticity seems to be such-and-such; but this is not to be taken as any proof of such ellipticity.*" But here again, on the other hand, our statement was made on good authority; for in 'Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India, Australia, made at the Ordnance Survey Office, Southampton,' by Captain A. R. Clarke, R.E., under the direction of Colonel Sir Henry James, R.E., F.R.S., published by order of the Secretary of State for War (1866), we find :—"In computing the figures of the meridians and of the equator for the several measured arcs of meridian, it is found that the equator is slightly elliptical, having the longer diameter of the ellipse in 15° 34′ East longitude."

Surely this is as much a proof that the equator is elliptical as is Newton's proof that the planets attract one another according to a particular law because such a law of attraction would explain the planetary motions. But the reason for this attack of Major Herschel's on our statement that Captain Clarke had found the equator to be elliptical is seen from a passage in his article in 'Nature' on Colonel Clarke's 'Geodesy.' There Major Herschel admits that "unhappily" his "attitude" is a "prejudiced" one, because, he says :—"We have regarded the earth, mentally, for so many years as an irregular spheroid, and all ellipsoids or other mathematically simple figures as mere conveniences, that we cannot bring to bear upon the exact determination of any particular one of these that intense curiosity which is necessary to sustain one in the search for the most probable." This is, of course, quite consistent with the practical man's well-known rule of working, not to trouble his head with "mathematical labyrinths," nor with "the method of potentials," nor to countenance as "a legitimate part of geodetical study" calculations of rise of sea-level due to submerged spheres of rocky matter, and the like, which Colonel Clarke seems to have been so guilty as to do.

In 'Nature' Major Herschel is annoyed that Colonel Clarke should have spoken of the method of coincidences as modern; and he is more angry when we do so. Colonel Clarke was in his treatise compelled to leave out part of the history of his subject; and Major Herschel says:—"It is impossible not to feel surprise that the existence even of the enormous body of work

which is thus passed *sub silentio* is not even mentioned." But with us he is more decided in his expressions; for he says that our having only referred to the names of Clairault, Kater, and Borda is carrying "originality in experimental research too far—even to the very verge of decency." But we were not writing a history of pendulum-observations, much less of geodetic survey; all we desired to do in our short paper was to give an account of certain experiments our students had made to determine (as we believe for the first time) the acceleration due to gravity in the capital of Japan. As a matter of fact, the names of the students who assisted us, given at the end of our paper, are the only names we need have mentioned; and we quite fail to see the indecency of our not having expanded a short account of a modern experiment into an historical treatise on geodetic survey.

V. Permit us to assure Major Herschel that the joint working of ourselves and our students in the laboratory for several years has not been of such a nature as to require their "demanding to know *why*" certain experiments resulted as they did. The reversible pendulums we employed were such as are usually found among the collection of physical apparatus at a university. Although costing several pounds, they were in workmanship probably much inferior to those used for very accurate pendulum-observations; and under such circumstances we are sure that better results can be obtained with a long wire pendulum.

Pianoforte-wire and a ball of cast iron would enable a pendulum 200 feet long to be easily experimented with in any high chimney or tower. If the ball had more than 3 inches diameter, air-friction would not practically exist, and the correction for buoyancy and for air-inertia could be accurately made. With a complete amplitude of 30 centimetres (the average swing backwards and forwards that we employed), the arc-correction would be too insignificant to mention, the average velocity of the ball would be less than two centimetres per second, so that centrifugal force would introduce no lengthening of the wire; indeed that would be the case even with the larger arc of a single vibration, 30 centimetres, which we used in our paper when calculating the maximum possible effect of centrifugal force. The length of the wire could be measured with great accuracy; alterations due to change of temperature might be accurately allowed for; and slight discrepancies as to the position of the centre of gravity of the ball would not sensibly affect the answer. The arc of such a pendulum, after swinging for some days, would probably not have become less than 2 or 3 centimetres, quite

arge enough for electric registration; and the total number of , long series of swings could, with an accuracy of a hun- lredth of a second, be ascertained by the second chronograph nethod we employed. It would take too long and trespass oo much on your space to compare the advantages derivable rom this method of getting the time of swing, using a ship's hronometer instead of the older method of coincidences, in vhich a clock was employed.

That we are not alone in finding difficulties in the use of he reversible pendulum, and that therefore some superior in- trument is not undesirable, is seen from Major Herschel's own ırticle in 'Nature,' where, in spite of his present disparaging emarks regarding our preference for our long wire pendulum, ıe says:—"The recently recognized fact that most, if not all, he modern absolute determinations with a reversion-pendulum ıre vitiated to a sensible but unknown extent, which can at est be approximately estimated, is noticed" in Colonel Clarke's 'Geodesy;' and he admits that that writer was not ınreasonable in excluding "the absolute determinations, both hose anterior to Baily's time and those of more recent date."

So anxious are we to consider every possible cause of error n our experiments, that we take this opportunity of mention- ng two others not alluded to in our paper—the one the earth's nagnetic action on the pendulum-wire during the short time n each swing that the recording electric current was passing ılong it, the other the heating of the wire by this current and he consequent elongation of the pendulum. It will be seen ıt once that the first effect will be to produce a very slight ıorizontal force acting in a fixed direction for the twenty-fifth f a second or so each time the wire at the bottom of the pen- lulum-bob was in the mercury, the effect of which will be to lter excessively little the direction, but not the amount, of the orce of gravity. As regards the second effect, since the re- istance of the 934·99 centimetres length of steel wire 0·45 mil- imetres in diameter could not be more than 8 ohms; and s the rest of the circuit, including the electro-magnet of the ecording instrument, was not less than 10 ohms, the heat de- 'eloped in the wire while the current was passing, even ssuming no heat to be lost by radiation and convection vhile the current was each time passing, could not, with hree Daniell's cells, have been sufficient to lengthen the wire y the hundredth of a millimetre; and as the wire would be horoughly cooled between each passage of the current, no ossible error could be introduced from an accumulation of ıeat.

As regards any error which it might be suggested could

have arisen from vibration of the point of support, we may mention that we screwed the rigid brass plate on which the steel knife-edge rested firmly to a principal beam of the roof of the Physical-Demonstration room, which was built with unexceptionally great rigidity on account of earthquakes, so common in Japan.

VI. Major Herschel ends his letter by saying that "the force of gravity at Tokio, in Japan, is known more certainly from the above formula than from the experiments recorded in the paper under review." Now, even assuming that this were true (a statement, however, with which we disagree entirely), that is no reason why our experiments should not have been published, since it is always important that results obtained by one method should be confirmed by those obtained by another. But he further says, the paper contains no means of checking the result experimentally obtained. To remove this misapprehension on his part, we sum up the numbers involved in the calculations:—

Length of steel wire, at 0° C., from knife-edge to circumference of ball	934·99	centims.
Diameter of steel wire	0·045	centim.
Weight of wire	11·6	grams.
Diameter of brass ball	8·20	centims.
Weight of brass ball	2352·2	grams.
Length of steel knife-edge about . .	4	centims.
Breadth of steel knife-edge about .	1	centim.
Depth of steel knife-edge about . .	0·5	,,
Weight of steel knife-edge about .	7·8	grams.
Periodic time of pendulum for the above length	6·1496	seconds.
Amplitude, or complete swing backwards and forwards	30	centims.
Value of g, pendulum being assumed a simple mathematical one (no corrections)	980·06	cent. per sec. per sec.
Value of g, pendulum regarded as a compound one (uncorrected otherwise)	979·58	,, ,,
Correction for arc	0·00	,, ,,
Correction for buoyancy	+0·16	,, ,,
Correction for air-resistance . . .	0·00	,, ,,
Correction for air-inertia	+0·08	,, ,,
Final value of g experimentally obtained for Tokio, Japan	979·82	,, ,,

Calculated Values of g for Tokio.

From the formula, section 222, p. 167, Thomson and Tait's 'Natural Philosophy,' published 1867	979·74	cent. per sec. per sec.
From the formula given on p. 21 of Dr. Everett's Units & Phys. Const., published 1879	979·803	" "
From the formula given by Major Herschel, published 1880 . . .	979·811	" "

In our paper we merely, as a rough approximation, took the first of these calculated values to the first place of decimals.

Might we again draw attention to an important point advocated in our paper, but which runs the risk of being missed altogether in this discussion. It was our simple proposal of an easy way of measuring the relative value of g over the earth. Let rigid pendulums be sold, each accompanied by a table giving the number of swings per hour at many different temperatures in London. An observer in Siberia, say, finds the number of swings per hour at an observed temperature, and from his table at once sees the value of g for the place relatively to that of London.

In conclusion might we venture to suggest that, whatever may be Major Herschel's opinion of our ability as experimenters, whatever may be the estimate he forms of the value of our results, the style and tone of his letter is hardly what is usually employed by scientific men when discussing an investigation patiently made and faithfully recorded.

We are, Gentlemen,
Faithfully yours,
W. E. Ayrton,
John Perry.

June 16th, 1880.

VII. *On Anomalous Dispersion in Incandescent Sodium Vapour.* *By* A. Kundt*.

THE first observation of an anomaly in the dispersion of light was, as is well known, made by Le Roux upon iodine vapour. The vapour of iodine, which chiefly transmits only the extreme blue and red rays, shows a stronger refraction for the red than for the blue rays. As then anomalous dispersion was discovered in fuchsine by Christiansen, and I

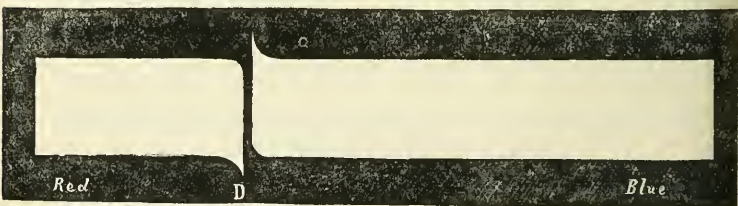
* Translated from Wiedemann's *Annalen*, 1880, No. 6, vol. x. pp. 321–325.

found the same phenomenon in many substances, and proved the relations between anomalies of dispersion and absorption of light and superficial colours, I expressed, in my third communication on anomalous dispersion *, the conjecture that the gases also, which sometimes possess so energetic an absorption for certain kinds of rays, must exhibit anomalies of dispersion in the vicinity of these rays. I added, however:—"But whether we shall ever succeed in demonstrating the refraction-anomalies in each single absorption-band of the gases and incandescent vapours, some of which show so great a number of thin absorption-lines, must be left undecided."

I have recently in fact observed in at least one incandescent vapour, that of sodium, a dispersion-anomaly in the vicinity of those rays which this vapour absorbs and emits. What holds good for sodium vapour, will at all events take place with all other absorbing gases and vapours, and, indeed, for the maximum of absorption of each of them. Herewith my previous conjecture is verified by experiment.

I was led in the following manner to the observation on sodium vapour:—While I, with Dr. Kohlrausch, the Assistant at the Institute, was making, for a lecture, the well-known experiment of the conversion of the bright sodium-line into a dark one, it struck us both that, when the absorbing sodium vapour was very dense and the dark line in the spectrum very broad, its upper and lower margins showed a peculiar rounding-out in the vicinity of the dark line. On closer examination I soon recognized that we had to do with a dispersion-anomaly, conditioned by the dispersion in the conical sodium-flame.

The spectrum with the dark line had, on the screen upon which it was thrown, the form shown in the annexed figure.



The experiment was arranged as follows:—By means of electric light a horizontal intensely bright spectrum was projected, through a prism with the edge vertical, upon the screen. In the path of the rays a Bunsen burner was placed, and with a small iron spoon a piece of sodium introduced into

* Pogg. *Ann.* vol. cxliv. p. 132 (1871).

it. If the spoon is brought exactly into the middle of the flame of the Bunsen burner, it is easy to maintain the flame above it as a cone of intensely yellow brightness. Now this cone acts like a prism with horizontal refracting angle turned upwards. Therefore, if the incandescent sodium vapour exhibits a dispersion, this cone of rays, which pass horizontally through it, must give a vertical spectrum (impure, it is true, on account of the conical shape). If the rays pass simultaneously through a glass prism with a vertical and the sodium prism with horizontal refracting angle, a spectrum is obtained which, if dispersion is present in the vapour, must have the shape above delineated*. As the refracting angle of the sodium prism lies above, the index of refraction of the vapour must be the highest for those rays which are most deflected downwards. The drawing shows that, in accordance with my investigations on solid bodies and liquids, the index of refraction rises much as the absorption-bands of the red side of the spectrum are approached, is lower on the green side of the dark line than on the other, and then rises again rapidly.

After the phenomenon was once recognized, I very often repeated the experiment; and when a very regularly conical sodium-flame of great intensity can be obtained, the anomaly in the refraction is very considerable. I have also, instead of sketching the phenomenon objectively on a screen, observed it subjectively with the telescope.

The above experiment, however, is successful only when the intensity of the sodium-flame is very great, such as is obtained by burning metallic sodium, and that for the following reason:—While the sodium-flame obtained by introducing a salt of sodium into the flame of a Bunsen burner, examined spectrally, shows two bright lines (the two D lines), the phenomenon is changed when a piece of sodium of the size of a pea is put into the burner. At first the two D lines come out distinctly; then, when the sodium begins to be vaporized in greater quantity, these lines widen considerably; with still greater density of the vapour they blend into one; and finally, upon this broad yellow band with fainter margins, there usually appear two fine black lines corresponding to the D lines. These dark lines are produced by the absorption of the cooler sodium vapour surrounding the bright sodium-flame. These phenomena have already been observed by Hankel † and Ciamician‡, and perhaps also by others.

The absorption-power changes correspondingly to the emis-

* Pogg. *Ann.* vol. cxliv. pp. 128-137 (1871).

† *Berichte der Leipziger Akademie,* 1871, p. 307.

‡ *Wien. Ber.* lxxviii. (1878), p. 887.

sion-power of the sodium-flame with increased density of the vapour. While a flame coloured by a sodium-salt, inserted as absorbing medium in the path of the rays, gives two sharp dark lines of absorption in the yellow of the spectrum, when the density of the vapour becomes greater these absorption-lines blend into a single broad band with fainter margins. Now with these bands the dispersion-anomaly appears distinctly visible, while with the narrow absorption-lines, though at any rate present, it is not recognizable, since here it is limited to an extremely small compass in close proximity to the two sides of each absorption-line.

If we could form a real prism of incandescent sodium-vapour, we should probably be able to observe indications of anomalous dispersion in the narrow absorption-lines, even with less density of the vapour. My endeavours, however, to convert the conical flame into a prismatic one by applying plates of glass or mica to its sides, led to no result. Just as little have I hitherto observed dispersion-anomalies in other incandescent vapours by introducing salts of the metals into the Bunsen burner; the density of these vapours, and consequently their absorption, is too slight for the method of observation I employed. With improved methods and very dense vapours the same phenomenon as in sodium-vapour will doubtless be obtained.

To the foregoing I add a remark which, so far as I know, has not yet been enunciated. Those solid and liquid bodies which exhibit for certain groups of rays strong absorption, and for neighbouring groups anomalous dispersion, possess, as I have previously shown, for the same groups a strong reflecting-power*.

As it is proved that incandescent gases exhibit anomalous dispersion in the vicinity of the rays which they strongly absorb, it must be assumed, according to the analogy of experience with liquid and solid bodies, that the gases also strongly reflect those rays which they strongly absorb and, consequently, emit. A sodium-flame, therefore, would reflect much more strongly rays of the number of vibrations of the D line than any other luminous rays of the spectrum, and therefore show a yellow surface. Experiments in proof of this inference (which presumably would present considerable difficulties) I have not yet instituted.

Before such experiments are available and the strength of the selective reflections of incandescent gases is in some measure quantitatively determined, it would be precipitate to

* *Conf.* Stokes, Pogg. *Ann.* xci. p. 158 (1854), and xcvi. p. 522 (1855).

build further conclusions on the existence of such selective reflection. I will merely point out that if the reflecting-power of incandescent gases for certain groups of rays is considerably greater than for all others, this will not be without importance for the spectroscopic investigation of heavenly bodies which, like comets for instance, emit partly their own, partly reflected light. If the light of such a body consists of single isolated groups or shows narrower brighter bands on a continuous dark spectrum, we are accustomed, according to our present knowledge, to assume that the light of this discontinuous spectrum is entirely and exclusively emitted by the body as a self-luminous one. If the body possesses a selective reflecting-power, the above conclusion is not at once admissible. One might even imagine, as the most extreme case, a non-luminous very dense mass of gas in our solar system, possessing selective absorption, and consequently for many separate groups of rays a power of selective reflection. Such a mass, intensely illuminated by the sun, would, without being self-luminous, show a discontinuous spectrum by reflection.

Strassburg, March 1880.

VIII. *Remarks on a Simplification of the Theory of Vibratory Motions.* *By* C. CELLÉRIER*.

THE motions in question are the oscillations of the particles on both sides of their positions of equilibrium—that is to say, those which constitute sound and light. To find their law on the most general hypothesis, the excursions and the molecular velocities at a fixed instant called the initial instant are supposed to be given; the unknowns are the projections of those excursions upon three rectangular axes at any instant whatever: they are functions of the time and of the position of the particle.

The equations of the motion are satisfied by taking for each of them a sum of terms of the form $a \cos \rho(p-st)$, in which t is the time, p the distance of the particle from a fixed plane, and a, ρ, s constants. The motion represented by one of these terms isolated is called a simple motion.

At the initial instant each of the terms reduces to $a \cos \rho p$; and the constants and the fixed plane corresponding to each can be arranged so that their sum shall reproduce the initial

* Translated from the *Archives des Sciences Physiques et Naturelles* of the *Bibliothèque Universelle*, June 5, 1880, pp. 549–553, having been communicated to the Société de Physique et d'Histoire naturelle of Geneva on June 3, 1880.

values of the excursion—that is to say, any given function f of the position of the point in space. For this we must have

$$f = \Sigma a \cos \rho p.$$

This mode of representation of a function is Fourier's formula.

The values of the unknowns thus found are sextuple integrals, an exact solution of the problem, but which give no idea of the general form of the motion. It is only after laborious transformations, supposing the initial disturbance included within a limited space, that we arrive at interpreting them so as to make evident the limited wave-form.

Beside this complication, Fourier's formula has another inconvenience: the integration with respect to some of the variables has, if we commence with them, an indeterminate result; and differentiation under the symbol of integration offers but little guarantee of accuracy.

Now these inconveniences may be avoided by substituting for Fourier's formula another, likewise representing an arbitrary function $f(x, y, z)$ of three indeterminates which may be regarded as rectangular coordinates of a variable point: the function is supposed $=0$ if the point is outside of a limited space designated by V. For the enunciation of the formula, we will denote by S a spherical surface having unity for radius, and for its centre the origin O; it shall be divided into elements ω; designating by H the position of any one of them, α, β, γ will be the cosines of the angles made by the straight line OH with the axes; P will denote a plane perpendicular to OH, cutting this straight line at a distance p from the origin, which distance will be taken as negative on the side opposite to H; lastly, we will put

$$\mathrm{F}(p) = \Sigma f(x', y', z')\omega',$$

the summation extending to all the elements ω' of the plane P, and x', y', z' being the coordinates of each.

Thus $\mathrm{F}(p)$ will be a function solely of p and the cosines α, β, γ. Let $\phi(p)$ be its second derivate with respect to p, in taking which α, β, γ are regarded as constants. The following is the formula sought:—

$$f(x, y, z) = -\frac{1}{8\pi^2}\Sigma(\alpha x + \beta y + \gamma z)\omega,$$

in which the symbol of summation extends to all the elements ω of the sphere S; α, β, γ correspond to each; x, y, z are the coordinates of any point in space.

The preceding expression comprises in the main four inte-

grations, all within finite limits and without indetermination; they can be carried out rigorously; and we can thus demonstrate that $f(x, y, z)$* is found again as its value.

If we express in this way the initial functions, and reduce them all to the single function corresponding to one and the same element ω, they will be found to be functions of a single coordinate $\alpha x+\beta y+\gamma z$, which is the abscissa of any point whatever, counted in a direction parallel to the fixed direction OH; moreover they will have values different from O only within a restricted region: for example, the abscissa must be comprised between $\pm\mu$, attributing to the volume V the form of a sphere of radius μ and placing the origin at the centre.

Now, in this case the equations of the motion are integrated immediately, the excursions are functions solely of the same abscissa, and the simple motion which results is as easy to find as if it were represented by a cosine; only it is of quite a different nature and composed of a limited plane wave. Let us call that plane which was at first carried through the origin perperdicular to OH the middle plane; and suppose it to be displaced parallel, with a constant velocity S. The disturbed region will at each instant be bounded by two planes parallel to that plane, taken on both sides at the distance μ.

The total motion results from the superposition of these plane waves; their middle planes form, at the end of the time t, the whole of the tangent planes to one and the same interior envelope, which is the surface of the waves; the motion is sensible only within a small thickness on both sides of the envelope; and it can be ascertained by a very simple reasoning that the prolongations of the plane waves outside of this limited region interfere with one another, at least at a notable distance from the centre of disturbance.

Either the complete integrals given by Poisson for an isotropic medium, or, in the case of a crystallized medium, the law of the motion for a point at a distance from the origin is also found with sufficient facility. The latter is found to depend on the radius of curvature of the apparent contour of the surface of the waves upon any plane which passes through one of its normals. Now it is remarkable that, if a secant plane parallel to the former be carried through the same normal, the product of the radius of curvature of the section into that of the contour is constant; besides, the radii of curvature of the surface of the waves are determined by those of the surface of the sixth order which represents the velocities; so that the formulæ no longer contain any thing unknown.

* For further details see the *Mémoires de la Société de Physique de Genève*, année 1880.

It is moreover immaterial whether we employ in the results Fresnel's representation of the surface of the waves or that deduced from Cauchy's theorem: the latter only is theoretically accurate; but I have before demonstrated that, on determining by experiment the constants which enter into it, it coincides with Fresnel's, and that with an exactness exceeding that of the observations.

It is remarkable that the transformation indicated above for Fourier's formula has no existence for a function of two variables. This fact is connected with another, viz. that there cannot exist a limited cylindrical wave, at least interiorly.

Supposing the initial disturbance circumscribed within an indefinite vertical cylinder, admitting moreover that the excursion and the velocity are at that instant the same for all points situated on one and the same vertical, this will certainly be the case during the whole motion; and this will have the form of a vertical cylindrical wave propagating itself horizontally in all directions; but the whole of the points interior to the cylinder will remain indefinitely in motion; so that it cannot be resolved into limited plane waves.

IX. *On a Simple Method of Galvanic Calibration of a Wire. By* V. Strouhal *and* C. Barus*.

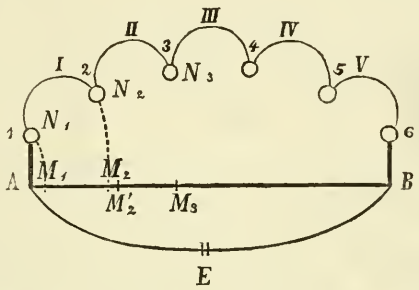
THE methods which are customarily used for the calibration of a wire (as, for example, in the Kirchhoff-Wheatstone bridge-combination) undoubtedly labour under the disadvantage that for carrying them out resistances are required beforehand which have been equalized in another way under certain simple conditions. In this way the accuracy of the desired result is made dependent on that of a previous equalization, whereby the errors to be expected in the calibre of the wire are placed in doubt by unavoidable errors in the equalization of the resistances, and so much the more the smaller they happen to be. Hence the importance of the problem for precise determinations by the otherwise so convenient bridge method constantly justifies the endeavour to get rid of this inconvenience as completely as possible, and, in addition, by the simplicity of the means propounded, to make the execution to the utmost convenient and facile.

The method described in the following, which we have repeatedly employed with advantage, is perfectly analogous to that which is usually applied, in the well-known manner, to

* Translated from Wiedemann's *Annalen*, 1880, No. 6, vol. x. pp. 326-330.

the calibration of a thermometer. It starts from the same idea on which the well-known Hockin-Matthiessen method of employing the bridge rests.

In the annexed figure let A N B and A M B be the two branches of the current in the known bridge-arrangement—A M B the wire to be calibrated, A N B a series of resistances, whose sum with any arrangement remains constant.



Now let M_1 and N_1, or M_2 and N_2, be two pairs of points of equal potential, and let y denote the resistance $N_1 N_2$, x the section $M_1 M_2$ of the wire; then

$$y = Cx,$$

where C is a constant (the " constant of sensitiveness ") which depends only on the sum of the resistances ANB. In fact, if W denotes the sum, and L the total length of the wire,

$$W = C \, . \, L.$$

Further, let a be the calibration-interval, so chosen that $\frac{L}{a} = n$ is a whole number.

Now, first of all, n *nearly equal* resistances are prepared. We had a number of tenths of a Siemens unit already at hand, which we had previously procured and could employ for the purpose of resistance-determinations. A German-silver wire, however, of suitable length and thickness can be taken and cut into n approximately equal parts, which it is then expedient to solder to pieces of amalgamated copper wire. Exact equalization is superfluous.

These n approximately equal resistances I, II, III, &c. are placed in a series one after another by the interposition of mercury-cups. We will assume $n = 5$. The above figure represents the arrangement for this case. The connexion with the extremities A and B of the wire is effected by means

of thick copper wire or plates. The calibration now takes place in the following manner:—

The contact N of the "bridge-wire" M N, passing through a sensitive mirror-galvanoscope, is put successively into the cups 1 and 2, and the corresponding positions M_1 and M_2, upon the wire A M B, of the contact are determined.

I and II are now exchanged, so that I comes into the place of II; the contact N is put successively into the cups 2 and 3; and again the corresponding positions M'_2 M_3 of the contact M are determined. We now move I further on, by making it and III change places, put the contact N successively into 3 and 4, and determine the positions M'_3 and M_4.

We proceed in this way until I has arrived at the place of the last resistance V.

We thus obtain, upon the bridge, the lengths $M_1 M_2$, $M'_2 M_3$, $M'_3 M_4$, &c., which are each proportional to the same resistance.

It is easily seen that, in fact, between this method of wire-calibration and the well-known method of thermometer-calibration a perfect analogy prevails. As in the latter a mercury thread of a determined volume and the approximate length $a=\frac{100}{n}$ is shifted in the thermometer-tube, and in the graduation of the thermometer its length is read off, consequently a quantity which, on the assumption of equal cross section of the tube, is proportional to that volume, so also in our method a wire possessing a determined resistance is shifted from place to place, and is read off in the division of the length, consequently likewise a quantity which, on the assumption of equal cross section of the wire to be calibrated, is proportional to that resistance. From the differences of equivalent lengths we infer in both cases respecting the calibre of the tube and the wire respectively. The fixed points between which the divisions are put in are, in the case of the thermometer, the freezing- and the boiling-point; in our case they are the two branching-points A and B.

Now, if a_1, a_2, a_3, &c. are the equivalent wire-lengths read off, then the mean length which, assuming a mean cross section of the calibrated wire, corresponds to the calibration-resistance is given by

$$\frac{a_1+a_2+a_3+\ldots+a_n}{n}.$$

As $a_1, a_2, \ldots$ deviate each but very little from the calibration-interval $a=\frac{L}{n}$, it is more practical for the calculation to bring

into it not the entire lengths $a_1, a_2, a_3, \ldots$, but only their (positive or negative) excesses over a. We therefore put

$$a_1 = a + \delta_1, \quad a_2 = a + \delta_2, \; \ldots \ldots \; a_n = a + \delta_n,$$

and in like manner

$$\frac{a_1 + a_2 + a_3 + \ldots + a_n}{n} = a + \alpha.$$

Then, more simply,

$$\alpha = \frac{\delta_1 + \delta_2 + \delta_3 + \ldots + \delta_n}{n};$$

and hence the table of corrections of the wire is:—

From 0 to a .... $\alpha - \delta_1$
„ a „ $2a$ .... $\alpha - \delta_2$,
„ $2a$ „ $3a$ .... $\alpha - \delta_3$
&c.

As a peculiar advantage of the method, we might insist especially upon the simplicity of the means. It has only one drawback, that many single placings have to be made, through which possible variations of temperature might enter disturbingly; but the work can be rapidly performed, since approximate positions are given by the nature of the thing itself. If only a moderately sensitive mirror-galvanometer be employed, the position-errors are of the same order as the errors of reading off. With our bridge, $2\frac{1}{4}$ metres long, a tenth of a millimetre in the placing was perfectly secure.

Doubtless other methods which have been proposed for calibrating the thermometer might, on the basis of our principle, be transferred to the galvanic calibration of a wire. The one above described, however, is certainly sufficient for all purposes.

Würzburg, Physical Institute,
April 10, 1880.

X. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. ix. p. 386.]

May 26, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Pre-Carboniferous Rocks of Charnwood Forest." (Part III. Conclusion.) By Rev. E. Hill, M.A., F.G.S., and Prof. T. G. Bonney, M.A., F.R.S., F.G.S.

In their former communications the authors had paid less atten-

tion (from want of time) to the northern part of the forest than to the rest. This district has during the last two years engaged their special attention. They had provisionally retained the name quartzite for the rocks exposed about Blackbrook &c., probably the lowest visible on the forest. This name proves to be inappropriate; and they propose to call the group, which contains much fine detrital volcanic material, the Blackbrook Series. They have also reason to believe that the anticlinal fault is less than was supposed, and that we have here a fairly unbroken base for the forest-rock already described. In this case there ought to be representatives of the great agglomeratic masses on the western side of the anticlinal (High Towers &c.). The authors believe that they have found these, though as much finer and more waterworn detritus, in the greenish grits above Longcliff and Buckhill. The authors also believe that they have succeeded in tracing a coarse agglomerate with slate fragments round about three fourths of the circumference of the forest. Further notes upon the district of Bardon Hill, Peldar Tor, and Sharpley are given; and the origin of the remarkable rock of the last, so like some of the Ardennes porphyroids, is discussed ; the authors believe it to be a volcanic tuff, altered by the passage of water or of acid gases. Descriptions of the microscopic structure of some of the rock fragments included in the coarse agglomerate and of some of the slates are given. Also a notice of two small outbursts of igneous rock, of the northern syenite type, previously unnoticed are mentioned.

2. "On the Geological Age of Central and West Cornwall." By J. H. Collins, Esq., F.G.S.

The author divided the stratified rocks of this district into four groups, as follows :—

1. *The Fowey Beds*, mostly soft shales or fissile sandstones, with some beds of roofing slate: no limestones or conglomerates. These beds cover an area of not less than eighty square miles, and contain numerous fragmentary fish-remains and other fossils, many as yet undetermined, the whole, however, indicating that the beds are either Lower Devonian or Upper Silurian. The strike of the beds is N.W. to S.E.; and they are estimated to be not less than 10,000 feet thick.

2. *The Ladock Beds,* consisting of slaty beds, sandy shales, sandstones, and conglomerates; no limestones and no fossils. They cover an area of more than 100 square miles to the west and south of St. Austell, strike from east to west, and overlie Lower Silurian rocks unconformably. They are estimated at from 1000 to 2000 feet thickness.

3. *The Lower Silurians* consist largely of slates and shales, with some very thick conglomerates (one being at least 2000 feet thick), some quartzites, and a few thin beds of black limestone. The quartzites and limestones have yielded fossils (chiefly Orthidæ) which are pronounced to be of Bala or Caradoc age by Davidson and others. The total thickness of these beds is estimated at

23,000 feet; and the fossils are found in the upper beds only. Instead of occupying only about 12 square miles, as shown on the Survey Maps, they extend over nearly 200 square miles, and reach southward beyond the Helford River, and westward to Marazion. The strike of these rocks is from north-east to south-west.

4. *The Ponsanooth Beds* occur beneath the Lower Silurians, and unconformable with them (strike north-west to south-east); they are often crystalline, and are estimated at 10,000 feet thickness.

Each of these formations has its own set of intrusive rocks; each has been contorted and in part denuded away before the deposition of its successor.

The various granitic bosses have been pushed through this already complex mass of stratified rocks without materially altering their strike, which does not in general coincide with the line of junction.

The chemical effects of the igneous intrusions are generally considerable, and somewhat proportioned to their relative bulk.

3. "On a Second Precambrian Group in the Malvern Hills." By C. Callaway, Esq., D.Sc., F.G.S.

These rocks occupy an area of about half a square mile on the east of the Herefordshire Beacon; they are compact, flinty "hornstones," very like some of the rocks at Lilleshall, in Shropshire, which belong to the newer Precambrian group of the Wrekin. The strike is not distinct, but probably is quite discordant from that of the subjacent gneissic rocks. As in Shropshire, so here Hollybush sandstone and *Dictyonema*-shales occur on the flank of the Precambrian mass, and each seems to have formed an island in the Lower-Silurian seas, which, during the formation of the May-Hill group, was depressed. In fact, in both regions the chief movements of upheaval, subsidence, and dislocation appear to have been contemporaneous. Thus they are very probably of the same age; and this probably is that of the Pebidians of St. David's, to some of which they have, lithologically, a very close resemblance.

June 9, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Occurrence of Marine Shells of existing Species at different Heights above the present Level of the Sea." By J. Gwyn Jeffreys, LL.D., F.R.S., Treas. G.S.

This paper resulted from the author's examination of the Mollusca procured during the expeditions of H.M.SS. 'Lightning' and 'Porcupine' in the North Atlantic. He stated that he found several species of shells living only at depths of not less than between 9000 and 10,000 feet, which species occurred in a fossil state in Calabria and Sicily at heights of more than 2000 feet, such depths and heights together exceeding the height of Mount Etna above

the present level of the Mediterranean. He then gave an account of the Post-Tertiary deposits in Europe, Asia, and North America, to show their various heights, and especially of the raised beach on Moel Tryfaen in Caernarvonshire, which was from 1170 to 1350 feet high. Some of the shells in that deposit were boreal and did not now live in the adjacent sea. The author stated that no shells of a peculiarly northern character had been noticed in the west or south of England. He then questioned the permanence and even the antiquity of the present oceanic basins, from a consideration not only of the fauna which now inhabits the greatest depths, but also of the extent of oscillation which had prevailed everywhere since the Tertiary period. A complete list of the Moel-Tryfaen fossils was given, to the number of 60, besides 3 distinct varieties, of which number 11 were arctic or northern, and the rest lived in Caernarvon Bay. All of these fossils were more or less fragmentary.

2. "On the Pre-Devonian Rocks of Bohemia." By J. E. Marr, Esq., B.A., F.G.S.

The author commenced with a brief notice of the Pre-Cambrian rocks, which are gneisses and schistose limestone with intrusive eclogite; over these lie unconformably green grits, ashes, breccias, hornstones (étage A of Barrande), which the author considers to represent the Harlech Group of Wales. Étage B is unconformable with this, but conformable with C, which contains the "primordial" fauna. D contains the colonies. E to H are Silurian, and more calcareous than those underlying them. The base of the group is unconformable with those beneath. The lithological characters of the various beds were described. The following are the associated igneous rocks—Granite, Quartz felsite, Porphyrite, Mica-trap, Diabase, Diorite, Eclogite. Of these brief descriptions were given. The author gave a comparison of the various shales with English deposits. The Pre-Cambrian Series much resemble the Dimetian and Pebidian of Wales, the latter being étage A; étage B, the Harlech; étage C, the Menevian, probably a deep-water deposit, as is indicated by the abnormal size of the eyes of its Trilobites; the lowest bed of étage D probably represents part of the Lingula Flags of Britain. D, α, 1, β seems to represent the Tremadoc shale of Britain, and, like it, contains pisolitic iron-ore. Representatives also of the Arenig and Bala beds are found. A slight unconformity marks the base of the Silurian. Three Graptolitic zones occur. The lowest, or *Diplograptus* zone, identical with the Birkhill shales, contains thirteen species of Graptolites; the next, or *Priodon* zone (four species), resembles the Brathay Flags; the upper, or *Colonies* zone (five species), resembles the Upper Coldwell Beds of the Lake-district. Above these follow representatives of Wenlock, Ludlow, and probably of the Passage beds. The author, with the evidence of these, discussed the "colonies" theory of M. Barrande, pointing to the non-

intermixture of species, notwithstanding the irregular repetition of the zones, the non-occurrence of these colony species in intermediate beds, and other reasons. The stratigraphy and palæontology of several of these colonies was discussed in detail, showing it to be more probable that their apparent intercalation with later faunas is due to repetition by faulting.

3. "On the Pre-Cambrian Rocks of the North-western and Central Highlands of Scotland" By Henry Hicks, Esq., M.D., F.G.S.

The author, after examination, considers the rocks of the following districts to be wholly or in part Pre-Cambrian:—

(1) *Glen Finnan, Loch Shiel to Caledonian Canal.*—In the former district the rocks are gneiss, often massive. In Glen Firmilee is a series which the author regards as newer and Pebidian. At Farofern are quartz rocks which the author identifies with those beneath the limestone in Glen Laggan, near Loch Maree, and probably of Silurian age. At Bannavie is a granite which the author considers to be Pre-Cambrian.

(2) *Fort William and Glen Nevis.*—In this district chloritic schists and gneiss occur, which the author regards as Pebidian.

(3) *Ballachulish, Glen Coe, and Black Mount.*—Chloritic schists and quartzites occur here, followed near Loch Leven unconformably by Silurian rocks. On the east of the Ardsheal peninsula there is granite which the author believes to be Pre-Cambrian. Going eastward from Ballachulish we have slates, probably of Silurian age. In Glencoe are granite-banded felsite, gneiss, breccia, resembling as a whole the rocks of the Welsh Arvonian group. Between the Black Mount and Loch Sullich are traces of a great Pre-Cambrian axis, bringing up the gneissic series; this is traceable also towards Glen Spean and Loch Laggan to the N.E.

(4) *Tyndrum to Callander.*—South and east of the former are gneisses and silvery mica-schists. Crystalline limestones and serpentines are associated near Loch Tay, resembling those in the Pebidian series of North Wales.

The author states that the Silurian (and Cambrian) rocks flank the Pre-Cambrian in lines from N.E. to S.W., and overlap Ben Ledi on the south side. Thus here, as elsewhere, subsequent denudation has removed enormous masses of the more recent rocks, only here and there leaving patches of these in folds along depressions in the old Pre-Cambrian floor.

XI. *Intelligence and Miscellaneous Articles.*

NOTE ON THE CONDUCTIVITY OF TOURMALINE CRYSTALS. BY GEORGE FRANCIS FITZGERALD, M.A., F.T.C.D.

In the Philosophical Magazine for July 1879, Professor Sylvanus Thompson and Dr. Oliver Lodge give the results of some very interesting experiments upon the unilateral conductivity of tour-

maline crystals for heat and electricity. Dr. Lodge had shown that an explanation of pyroelectricity might be given if such crystals possessed a unilateral conductivity for electricity. A body is said to possess unilateral conductivity for any thing if it conducts better in one direction than in the opposite one—as, for example, a tube with a series of funnels in it all turned the same way for fluids, and apparently, in the case of Geissler's tubes, for electricity also. The result of their experiments was that tourmaline crystals do possess a unilateral conductivity for heat *as long as their temperature is variable*, and similarly for electricity *as long as the temperature varies.* The first of these facts is an important and valuable increase of our knowledge; but the latter, as they point out, is of course only due to the already known electromotive force which constitutes their pyroelectric properties. They seem to have been dissatisfied with these results; for they had hoped to discover unilateral electric conductivity independently of changes of temperature. They do not seem to have noticed that what analogy should have led them to look for was unilateral conductivity *during changes of intensity of the current.* It is to be hoped that, as they possess a very fine specimen of tourmaline, they will continue their investigations into this point. In the meanwhile it may be worth noticing a mechanical illustration of how this might be connected with pyroelectricity. Suppose a wire carrying a current, surrounded by a number of magnets, and that a majority of them pointed in one direction round the wire, and that each was kept in its place by a spring. On passing a current through the wire, all the magnets that did not point round it in a particular direction would tend to set themselves in this direction; and during changes of intensity of the current, work would be done against or by the springs. If the current passed in such a direction that the majority of the magnets were set so as to remain unchanged, there would be less work done by changes of intensity than if the current were in the opposite direction; and this would give rise to an apparent unilateral conductivity. I say "apparent," because the weakening of the current is due to an inverse electromotive force, and not to a true increase of the resistance. The same effect would be produced by supposing a majority of the magnets turned in the same direction along the magnet, and kept in position by two springs, one on each side, but one stronger than the other, when of course a current would have to do more work in turning them to one side than to the other, so that in this case also there would be apparent unilateral conductivity during variations of the current.

Now, suppose that the proportion of polarized magnets or their strength depended on the temperature of the system. It is then evident that during changes of temperature there would be changes in the numbers or strengths of the polarized magnets: either would produce an electromotive force in the wire during the change. Hence the phenomena of pyroelectricity would be manifested by such a system. I put these forward merely as illustrations, not

supposing that the structure of tourmaline is necessarily at all *like* either of them; but there are generally great analogies between different systems exhibiting the same phenomena, and an illustration gives us a concrete stepping-stone to found our conceptions on during the difficult transit to the abstract.

The passage of a current through an iron wire is accompanied by the production of a series of magnetic elements round it; and the effect of this has been noticed as causing an apparent change of resistance during changes of the current; but as there is no want of symmetry in the wire, there is no apparent unilateral conductivity.—*Scientific Proceedings of the Royal Dublin Society*, Jan. 19th, 1880.

ON A SIMPLE METHOD OF IDENTIFYING A SUBMERGED TELEGRAPH-CABLE WITHOUT CUTTING IT. BY W. P. JOHNSTONE, ESQ.

To the Editors of the Philosophical Magazine and Journal.

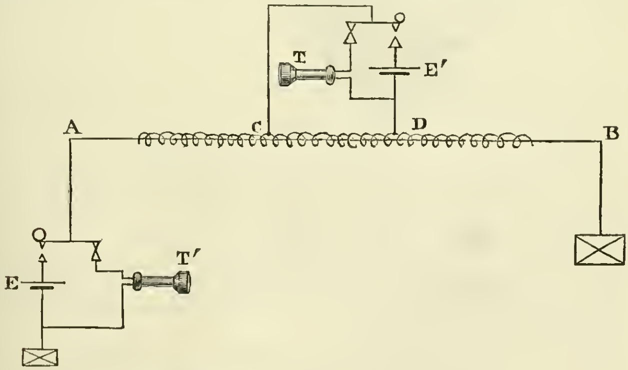
Electrician's Office, Alipore, Calcutta,
3rd May, 1880.

GENTLEMEN,

In continuation of my letter dated 27th April, 1880, I send you now a Postscript, which kindly publish with the Paper*, and oblige

Yours faithfully,
LOUIS SCHWENDLER.

P.S.—The following diagram represents a simple method for communication between the repairing-boat and the shore.



The telephone T on the boat is worked as before explained; and the telephone T′ on the shore is worked by the currents induced in the copper conductor by the currents sent into the guards from the

* We were unable to comply with our Correspondent's request, the Postscript having reached us after the issue of the June Number.—EDS.

battery E′ on board the repairing-boat. This method will always be found of great use in all cable expeditions, as it can be applied without difficulty to the longest cable.

COSMICAL DETERMINATION OF JOULE'S EQUIVALENT. BY PLINY EARLE CHASE, LL.D., PROFESSOR OF PHILOSOPHY IN HAVERFORD COLLEGE*.

In estimating heat of dissociation, Pfaundler has shown† that the mean should be taken between the temperatures of incipient and of complete dissociation. On this principle, in expressing the temperature of water-crystallization we should have regard to all stages of the expansion in molecular rearrangement, and take the mean (36°·6 F. = 2°·6) between the temperatures of greatest density (4°·6) and of complete crystallization (0°·6). So long as water continues to condense, its tendencies are centripetal and polar; while it is expanding, they are centrifugal and equatorial. The thermodynamic relations between heat and work should be shown in the comparative motions and temperatures of polar and equatorial waters as surely, and with as abundant facilities for accurate measurement, as in the experiments of the laboratory or in the processes of the workshop.

Johnston's Physical Atlas gives 82°·6 F. (28°·1 C.) as the mean temperature of the oceanic warmth-equator. This indicates a polar-equatorial difference of 82°·6 to 35°·6 F. = 47 J, or 28°·1 to 2° C. = 26·1 calories. The difference in gravitating measure may be readily deduced from the difference of motion. The velocity of equatorial rotation is 1525·78 feet, which represents a virtual fall of $\left(\frac{1525\cdot78}{32\cdot088}\right)^2 \times 16\cdot044$ ft. = 47 J. Hence we find J = 771·816 foot-pounds; calorie = 423·44 kilogrammetres.

ON SUBSTANCES POSSESSING THE POWER OF DEVELOPING THE LATENT PHOTOGRAPHIC IMAGE. BY M. CAREY LEA, PHILADELPHIA.

About three years since, I communicated to Silliman's American Journal the results of a long series of studies on development. At the time when these were undertaken there were but four substances known to possess the power of development:—ferrous sulphate, gallic acid, and pyrogallol, which had been long known to have this property; and hæmatoxylene, which I had some years before added to the number.

The studies made three years ago prove that the power of development, so far from being possessed by this small number of substances only, extends to a large number of chemical compounds, and is exhibited by many cuprous salts, by several vegetable acids,

* Communicated to the American Philosophical Society, April 16, 1880.

† Pogg. *Ann.* 1867, cxxxi. p. 603.

glucosides, &c. But the most curious result was obtained with ferrous salts. It was known that ferrous sulphate, though a powerful developer in the so-called "wet development" (*i. e.* development in presence of a soluble silver-salt), had no power whatever for those developments in which no soluble silver-salt was present, and where the development was to be made at the expense of the film itself. I was able to show that ferrous oxide combined with almost with any organic acid possessed this power of forming a visible image at the expense of the film. So that a solution of ferrous sulphate, by mixing with one of an alkaline oxalate, succinate, salicylate, &c., immediately acquires the power of development. Ferrous oxalate exhibits the power of development to a degree so remarkable that it seems likely to displace the older methods.

The study of the subject was resumed during the past winter, and with the result of ascertaining that this power of development was not limited to the organic salts of ferrous oxide, but was possessed by many of its inorganic compounds. It certainly has never been suspected that such compounds as *ferrous phosphate*, *ferrous borate*, *ferrous sulphite*, *ferrous hyposulphite*, &c. possessed the power of development; but this they undoubtedly do, and not in any uncertain way. On the contrary, some of these compounds are among the most powerful of all known developing agents, equalling, or possibly even excelling, ferrous oxalate in this respect; so that it is far from impossible that some of them may pass into technical use in preference to those now employed.

Some of these ferrous salts, especially the phosphate, sulphite, and borate, are, like the oxalate, insoluble in water, and therefore need to be got into solution. As these salts are not, like the oxalate, soluble in the corresponding alkaline salt, at least not to any useful extent, it becomes necessary to find an appropriate solvent. The most available solvents are solutions of ammonium and potassium oxalate, and of ammonium and sodium tartrate. Of these, the first have the material advantage that the ferrous salts remain permanently in solution, whereas with ammonium and sodium tartrate they are apt gradually to be precipitated.

As ferrous oxalate is a powerful developer, the question immediately presented itself whether the developing-power exhibited, for instance, by ferrous phosphate dissolved in ammonium oxalate, might not be due to the formation of ferrous oxalate. But several reactions contradict this supposition. When a hot solution of ammonium (neutral) oxalate is fully saturated with ferrous phosphate, a precipitate separates in cooling; and this precipitate is not ferrous oxalate but ferrous phosphate. Again, ferrous phosphate exhibits powerful developing-properties when dissolved in sodic or ammonic tartrate. This reaction, however, is not in itself decisive, inasmuch as I find that ferrous tartrate has itself developing-properties. But as ferrous phosphate is to some extent soluble in a solution of ferrous sulphate, and as ferrous sulphate (in the form of development here under consideration, namely in the absence of a soluble

silver salt) is wholly without developing-power, an opportunity offered itself of testing the question. And it proved that a solution obtained by adding one of disodic phosphate to one of ferrous sulphate until a permanent precipitate began to form, undoubtedly possessed developing-powers, though in a less degree.

The number of ferrous salts capable of developing the latent image is very considerable. Singular anomalies are often shown: a given salt prepared in one way may develop, while prepared in another it may have no such power. Nor is it possible to form an opinion beforehand as to whether a given compound of ferrous oxide will exhibit this power or not: compounds nearly allied do not exhibit analogies in this respect. For example, ferrous phosphate and ferrous metaphosphate are active developers, while ferrous pyrophosphate has no similar power.

Among other ferrous salts possessing more or less developing-power may be mentioned ferrous hyposulphite (hydrosulphate), ammonio-chloride, acetate, antimonio-tartrate, &c. Ferrous formiate, which might naturally be expected to be a powerful developer, is almost, though not entirely, destitute of the property. The most active agents found were ferrous borate, phosphate, sulphite, and oxalate, respectively dissolved, the phosphate in neutral ammonium oxalate, the others in neutral potassium oxalate.—Silliman's *American Journal*, June 1880.

NOTE ON THE CONSTRUCTION OF GUARD-RING ELECTROMETERS. BY GEORGE FRANCIS FITZGERALD, M.A., F.T.C.D.

Guard-ring electrometers have usually been constructed with an aluminium disk, for the sake of lightness, surrounded by a guard-ring of brass. It is essential for the accuracy of the calculation of the absolute values of capacity and quantity made with them that the electricity should be as uniformly distributed as possible on the surface of the disk and guard. It is for the sake of producing a uniform distribution on the disk that the guard is added. Hence any arrangement which disturbs this uniformity of distribution is to be avoided. Now, whether the contact of dissimilar metals in itself produces an appreciable difference of potential between them, or whether it is the air near different metals that is at different potentials, there is no doubt that when the plates of an accumulator are of different metals there is an appreciable accumulation of electricity upon them. Consequently in the guard-ring electrometer the distribution of electricity on the aluminium disk cannot be the same as on the brass guard connected with it. It might seem as if the other plate should be of the same material; but as it is generally easy to apply the differential method of measurement, a constant, even though unknown, difference of potential between the plates is of no consequence.—*Scientific Proceedings of the Royal Dublin Society*, Jan. 19th, 1880.

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AND
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[FIFTH SERIES.]

AUGUST 1880.

XII. *On the Resultant of a large Number of Vibrations of the same Pitch and of arbitrary Phase.* By LORD RAYLEIGH, F.R.S., *Professor of Experimental Physics in the University of Cambridge**.

VERDET†, in an investigation upon this subject, has arrived at the conclusion that the resultant of n vibrations of unit amplitude and arbitrary phase approaches the definite value $\sqrt{n}$ when n is very great. It can be shown‡, however, that this conclusion is inaccurate, and that the resultant tends to no definite value, however great the number of components may be.

But there is a modified form of the question, which admits of a definite answer, and was perhaps vaguely before Verdet's mind. If we inquire what is the *average* intensity in a great number of cases, or, in the language of the theory of probabilities, what is the *expectation* of intensity in a single case of composition, we shall find that the result is that assigned by Verdet, namely n.

A simple but instructive variation of the problem may be obtained by supposing the possible phases limited to *two opposite* phases, in which case it is convenient to discard the idea of phase altogether, and to regard the amplitudes as at random positive or negative. If all the signs are the same, the resultant intensity is n^2; if, on the other hand, there are as many positive as negative, the result is zero. But although the intensity may range from 0 to n^2, the smaller values are much more *probable* than the greater; and to calculate the ex-

* Communicated by the Author.
† *Leçons d'Optique physique*, t. i. p. 297.
‡ Math. Soc. Proc. May 1871.

pectation of intensity, these different degrees of probability must be taken into account. By well-known rules the expression for the expectation is

$$\frac{1}{2^n}\left\{1\,.\,n^2+n\,.\,(n-2)^2+\frac{n(n-1)}{1\,.\,2}(n-4)^2 + \frac{n(n-1)(n-2)}{1\,.\,2\,.\,3}(n-6)^2+\dots\right\}.$$

The value of the series, which is to be continued so long as the terms are finite, is simply n, as may be proved by comparison of coefficients of x^2 in the equivalent forms

$$(e^x+e^{-x})^n=2^n(1+\tfrac{1}{2}x^2+\dots)^n = e^{nx}+ne^{(n-2)x}+\frac{n(n-1)}{1\,.\,2}e^{(n-4)x}+\dots.$$

The expectation of intensity is therefore n, and this whether n be great or small.

In the more general problem, where the phases are distributed at random over the complete period, the expression for the expectation of intensity is

$$\int_0^{2\pi}\int_0^{2\pi}\int_0^{2\pi}\dots\frac{d\theta}{2\pi}\frac{d\theta'}{2\pi}\frac{d\theta''}{2\pi}\dots[(\cos\theta+\cos\theta'+\cos\theta''+\dots)^2 + (\sin\theta+\sin\theta'+\sin\theta''+\dots)^2].$$

If we effect the integration with respect to θ, we get

$$\int_0^{2\pi}\int_0^{2\pi}\dots\frac{d\theta'}{2\pi}\frac{d\theta''}{2\pi}\dots[1+(\cos\theta'+\cos\theta''+\dots)^2 + (\sin\theta'+\sin\theta''+\dots)^2].$$

Continuing the process by successive integrations with respect to θ', θ'', ..., we see that, as before, the expectation of intensity is n.

So far there is no difficulty; but a complete investigation of this subject involves an estimate of the relative probabilities of resultants lying between assigned limits of magnitude. For example, we ought to be able to say what is the probability that the intensity due to a large number (n) of equal components is less than $\frac{1}{2}n$. It will be convenient to begin by taking the problem under the restriction that the phases are of two opposite kinds only. When this has been dealt with, we shall not find much difficulty in extending our investigation to phases entirely arbitrary.

By Bernoulli's theorem* we find that the probability that

* Todhunter's 'History of the Theory of Probability,' § 993.

of n vibrations, which are at random positive or negative, the number of positive vibrations lies between

$$\tfrac{1}{2}n-\tau\surd(\tfrac{1}{2}n) \text{ and } \tfrac{1}{2}n+\tau\surd(\tfrac{1}{2}n)$$

is, when n is great,

$$\frac{2}{\surd\pi}\int_0^\tau e^{-t^2}dt,$$

where $\tau=r\surd(2n)$, and r must not surpass $\surd n$ in order of magnitude. In the extreme cases the amplitude is $\pm 2\tau\surd(\frac{1}{2}n)$, and the intensity is $2\tau^2 n$. Thus, if we put $\tau=\frac{1}{2}$, we see that the chance of intensity less than $\frac{1}{2}n$ is

$$\frac{2}{\surd\pi}\int_0^{\frac{1}{2}} e^{-t^2}dt=\cdot 5205\,;$$

so that however great n may be, there is always more than an even chance that the intensity will be less than $\frac{1}{2}n$. This, of course, is inconsistent with any such tendency to close upon the value n as Verdet supposes.

From the tables of the definite integral, given in De Morgan's 'Differential Calculus,' p. 657, we may find the probabilities of intensities less than any assigned values. The probility of intensity less than $\frac{1}{8}n$ is $\cdot 2764$.

Again, the chance that in a series n the number of positive vibrations lies between

$$\tfrac{1}{2}n+\tau\surd(\tfrac{1}{2}n) \text{ and } \tfrac{1}{2}n+(\tau+\delta\tau)\surd(\tfrac{1}{2}n)$$

is

$$\frac{1}{\surd\pi}e^{-\tau^2}\delta\tau,$$

which expresses accordingly the chance of a positive amplitude lying between

$$2\tau\surd(\tfrac{1}{2}n) \text{ and } 2(\tau+\delta\tau)\surd(\tfrac{1}{2}n).$$

Let these limits be called x and $x+\delta x$, so that $\tau=x/\surd(2n)$; then the chance of amplitude between x and $x+\delta x$ is

$$\frac{1}{\surd(2\pi n)}e^{-\frac{x^2}{2n}}\delta x.$$

The expectation of intensity is expressed by

$$\frac{1}{\surd(2\pi n)}\int_{-\infty}^{+\infty} e^{-\frac{x^2}{2n}}x^2\,dx=n,$$

as before.

It will be convenient in what follows to consider the vibrations to be represented by lines (of unit length) drawn from a fixed point O, the intersection of rectangular axes Ox and Oy.

If n of these lines be taken at random in the directions $\pm x$, the probability of resultants also along $\pm x$, and of various magnitudes, is given by preceding expressions. We will now suppose that $\frac{1}{2}n$ are distributed at random along $\pm x$, and $\frac{1}{2}n$ along $\pm y$, and inquire into the probabilities of the various resultants. The probability that the end of the representative line, or, as we may consider it, the representative *point*, lies in the rectangle $dx\,dy$ is evidently

$$\frac{1}{\pi n} e^{-\frac{x^2+y^2}{n}} dx\,dy.$$

Substituting polar coordinates r, θ and integrating with respect to θ, we see that the probability of the representative point of the resultant lying between the circles r and $r+dr$ is

$$\frac{2}{n} e^{-\frac{r^2}{n}} r\,dr.$$

This is therefore the probability of a resultant vibration with amplitude between the values r and $r+dr$. In this case there are n components distributed in four rectangular directions; and we have supposed that $\frac{1}{2}n$ exactly are distributed along $\pm x$, and $\frac{1}{2}n$ along $\pm y$. It is important to remove this restriction, and to show that the result is the same when the distribution is perfectly arbitrary in respect to all four directions.

In order to see this, let us suppose that $\frac{1}{2}n+m$ are distributed along $\pm x$ and $\frac{1}{2}n-m$ along $\pm y$, and imagine how far the result is influenced by the value of m. The chance of the representative point of the resultant lying in the rectangle $dx\,dy$ is now expressed by

$$\frac{1}{\pi\sqrt{(n^2-4m^2)}} e^{-\frac{x^2}{n+2m}-\frac{y^2}{n-2m}} dx\,dy$$

$$=\frac{1}{\pi\sqrt{(n^2-4m^2)}} e^{-\frac{n(x^2+y^2)+2m(y^2-x^2)}{n^2-4m^2}} dx\,dy$$

$$=\frac{1}{\pi\sqrt{(n^2-4m^2)}} e^{-\frac{nr^2}{n^2-4m^2}} e^{-\frac{2mr^2}{n^2-4m^2}\cos 2\theta} r\,dr\,d\theta.$$

Also

$$\int_0^{2\pi} e^{-\frac{2mr^2\cos 2\theta}{n^2-4m^2}} d\theta = 2\pi\left\{1+\frac{m^2r^4}{(n^2-4m^2)^2}+\dots\right\},$$

as we find on expanding the exponential and integrating. Thus the chance of the representative point lying between the circles r and $r+dr$ is

$$\frac{2r\,dr}{\sqrt{(n^2-4m^2)}} e^{-\frac{nr^2}{n^2-4m^2}}\left\{1+\frac{m^2r^4}{(n^2-4m^2)^2}+\dots\right\}.$$

Now, if the distribution be entirely at random, all the values of m of which there is a finite probability are of order not higher than $\sqrt{n}$, n being treated as infinite. But if m be of this order, the above expression is the same as if m were zero, and thus it makes no difference whether the numbers of components along $\pm x$ and along $\pm y$ are limited to be equal or not. The previous result, viz.

$$\frac{2}{n} e^{-\frac{r^2}{n}} r\, dr,$$

is accordingly applicable to a thoroughly arbitrary distribution among the four rectangular directions.

The next point to notice is that the result is symmetrical, and independent of the direction of the axes, so long as they are rectangular, from which we may conclude that it has a still higher generality. If a total of n components, to be distributed along one set of rectangular axes, be divided into any number of groups, it makes no difference whether we first obtain the probabilities of various resultants of the groups separately and afterwards of the final resultant, or whether we regard the whole n as one group. But the resultant of each group is the same, notwithstanding a change in the system of rectangular axes; so that the probabilities of various resultants are unaltered, whether we suppose the whole number of components restricted to one set of rectangular axes or divided in any manner between any number of sets of axes. This last state of things, however, is equivalent to no restriction at all; and we thus arrive at the important conclusion that, if n unit vibrations of equal pitch and of arbitrary phases be compounded, the probability of a resultant intermediate in amplitude between r and $r+dr$ is

$$\frac{2}{n} e^{-\frac{r^2}{n}} r\, dr,$$

a similar result applying, of course, in the case of any other vector quantities.

The probability of a resultant of amplitude less than r is

$$\int_0^r \frac{2}{n} e^{-\frac{r^2}{n}} r\, dr = 1 - e^{-\frac{r^2}{n}};$$

or, which is the same thing, the probability of a resultant greater than r is

$$e^{-\frac{r^2}{n}}.$$

The following table gives the probabilities of intensities less

than the fractions of n named in the first column. For example, the probability of intensity less than n is ·6321.

·05	·0488	·80	·5506
·10	·0952	1·00	·6321
·20	·1813	1·50	·7768
·40	·3296	2·00	·8647
·60	·4512	3·00	·9502

It will be seen that, however great n may be, there is a reasonable chance of considerable relative fluctuations of intensity in consecutive trials.

The *average* intensity, expressed by

$$\int_0^\infty \frac{2}{n} e^{-\frac{r^2}{n}} . r^2 . r\, dr,$$

is, as we have seen already, equal to n.

If the amplitude of each component be α, instead of unity, as we have hitherto supposed for brevity, the probability of a resultant amplitude between r and $r+dr$ is

$$\frac{2}{n\alpha^2} e^{-\frac{r^2}{n\alpha^2}} r\, dr.$$

The result is therefore in all respects the same as if, for example, the amplitude of the components had been $\frac{1}{2}\alpha$ and their number equal to $4n$. From this we see that the law is not altered, even if the components have different amplitudes, provided always that the whole number of each kind is very great; so that if there be n components of amplitude α, n' of amplitude β, and so on, the probability of a resultant between r and $r+dr$ is

$$\frac{2}{n\alpha^2+n'\beta^2+\ldots} e^{-\frac{r^2}{n\alpha^2+n'\beta^2+\ldots}} r\, dr.$$

The conclusion that the resultant of a large number of independent sounds is practically, and to a considerable extent, uncertain may appear paradoxical; but its truth, I imagine, cannot be disputed. Perhaps even the appearance of paradox will be removed if we remember that with two sounds of equal intensity the degree of uncertainty is far greater, as is evidenced in the familiar experiment with tuning-forks in approximate unison. That the beats should not be altogether obliterated by a multiplication of sources can hardly be thought surprising.

June 1880.

XIII. *The Vibrations of a Film in reference to the Phoneidoscope.* By WALTER BAILY, *M.A.**

[Plate I.]

THE object of this paper is to consider the superposition of several systems of waves in a plane film, when the vibrations are perpendicular to the film, and the wave-lengths of all the systems are the same, with the view of discovering (1) what combinations of systems give simple results in an infinite film, (2) which of these combinations can exist in a finite film, and (3) which of the latter can account for appearances presented in the phoneidoscope.

Let us examine the case of a plane uniform film of infinite extent traversed by three systems of waves with straight fronts, the vibrations being perpendicular to the film, and the wave-length of each system being the same.

Draw (fig. 1) Ac, Bc, Cc meeting in c; and let $\angle \mathrm{B}c\mathrm{C}=2\alpha$; $\angle \mathrm{C}c\mathrm{A} = 2\beta$; $\angle \mathrm{A}c\mathrm{B} = 2\gamma$. Take $c\mathrm{B} = c\mathrm{C} = \lambda$. Draw Q$c$R $\perp$ Ac; RBP $\perp$ Bc; PCQ $\perp$ Cc. Join Pc, and draw Qb and Rb, bisecting $\angle$s PQR and QRP. Draw $ca \parallel \mathrm{Q}b$, $cm \perp \mathrm{R}b$, $an \perp \mathrm{R}c$. It may be easily shown that $\angle cab=\alpha$, $\angle abc=\beta$, $\angle bca=\gamma$;

$$cm=\frac{\lambda}{2\sin\gamma},\quad an=\frac{\lambda\cos\beta}{2\sin\gamma\sin\alpha},\quad ca=\frac{\lambda}{2\sin\gamma\sin\alpha}.$$

Let λ be the wave-length; Ac, Bc, Cc the directions of the three systems of waves; h, k, l their amplitudes; and d, e, f their phases at some point. The values of $(e-f)$, $(f-d)$, $(d-e)$ are independent of time, and depend only on the position of the point chosen. QR, RP, PQ are wave-fronts; for they are $\perp$ to the directions of the waves.

Consider the line Pc. Every point on this line is equidistant from the wave-fronts RP and PQ, so that $(e-f)$ is constant along Pc. Similarly $(d-e)$ is constant along ab, and $(f-d)$ is constant along Rb; and as we may shift the point c, it follows that along any lines $\parallel$ Pb, Qb, and Rb we have $(e-f)$, $(d-e)$, and $(f-d)$ constant respectively. Hence $(f-d)$ is constant along ca.

The value of e at c differs by 2π from its value along PQ, since B$c=\lambda$. Hence the value of $(d-e)$ at c differs by 2π from its value along ab. Similarly the values of $(f-d)$ at b, and of $(e-f)$ at a differ by 2π from the corresponding values along ca and bc.

Now the wave-fronts Rc, cB, BR will reach a simultane-

* Communicated by the Physical Society, having been read at the meeting on June 12.

ously after crossing distances $= an$. Hence the difference of phase of the whole vibration at c and that at a

$$=\frac{an}{\lambda}2\pi=\frac{\pi\cos\beta}{\sin\gamma\sin\alpha}.$$

Draw (fig. 2) a series of straight lines $\parallel ab$ at distances $\frac{\lambda}{2\sin\gamma}$ apart. Along each of these lines $(d-e)$ is constant; and its value changes by 2π in passing from any line to the next. Draw another series $\parallel bc$ at distances $\frac{\lambda}{2\sin\alpha}$ apart. These lines will have similar properties with respect to $(e-f)$. Straight lines drawn through the intersections of these two series will form a series $\parallel ca$, at distances $\frac{\lambda}{2\sin\beta}$ apart, and having similar properties with respect to $(f-d)$. At all the intersections $(d-e)$, $(e-f)$, $(f-d)$ have the same values respectively; and therefore at all the intersections the amplitude of the vibration of the film is the same.

We can find a line such that $(d-e)$ is a multiple of 2π, and another line such that $(e-f)$ is a multiple of 2π. At their intersection $(f-d)$ will be also a multiple of 2π. Such a point is a ventral segment. Suppose (fig. 2) the lines to be moved until one of the intersections lies on a ventral segment; then all the intersections are ventral segments.

Now in a Δ with ventral segments at its $\angle$s, let p, q, r be the distances of a point from the sides of the Δ. Let p', q', r' be the differences between the values of $(e-f)$, $(f-d)$, $(d-e)$ at the point and on the respective sides. Then

$$p' : 2\pi = p : \frac{\lambda}{2\sin\alpha};$$

hence

$$p=\frac{p'\lambda}{4\pi\sin\alpha},\quad q=\frac{q'\lambda}{4\pi\sin\beta},\quad r=\frac{r'\lambda}{4\pi\sin\gamma}.$$

The displacement of the film at the point at any time may be expressed as

$$h\cos(d+\tau)+k\cos(e+\tau)+l\cos(f+\tau),$$

where τ is proportional to the time after the given moment. The maximum displacement will be

$$\sqrt{\{h^2+k^2+l^2+2kl\cos p'+2lh\cos q'+2hk\cos r'\}};$$

that is,

$$\sqrt{\left\{4s^2-4kl\sin^2\frac{p'}{2}-4lh\sin^2\frac{q'}{2}-4hk\sin^2\frac{r'}{2}\right\}},$$

where

$$2s = h + k + l.$$

This expression vanishes when

$$\sin^2\frac{p'}{2} = \frac{s(s-h)}{kl}, \quad \sin^2\frac{q'}{2} = \frac{s(s-k)}{lh}, \quad \sin^2\frac{r'}{2} = \frac{s(s-l)}{hk}.$$

These values show that there is a node in each triangle the position of which depends on the relative amplitudes of the waves. If one of the amplitudes (say l) is very nearly equal to the sum of the other two, $(s-l)$ is very small, and therefore r' is very small, and consequently r is very small; the result is that the nodes lie in pairs very near together. When one amplitude equals the sum of the other two, the pairs of nodes coalesce; and when one amplitude is greater than the sum of the other two, there can be no node.

In the particular case in which the directions of the waves are inclined at angles of 120° to one another, and the amplitudes are equal, the triangles in fig. 2 become equilateral, with their sides $= \frac{2\lambda}{3}$, and the nodes are equidistant from the ventral segments. Also the difference of phase between successive angles of the triangles is 120°. In fig. 3 let the dots represent the nodes and the numbers the ventral segments. Then all the *ones* move together, and so do all the *twos*, and also all the *threes*, the difference of phase between the different sets being 120°.

This last case may be investigated algebraically. Let $h =$ the amplitude of each wave, v the wave-velocity, t the time, x, y and r, θ the coordinates of a point in the film, and z the displacement at that point, at the time t. Take a ventral segment as origin, and the direction of one of the waves as axis of x. Then

$$\begin{aligned} h^{-1}z = &\cos\left[2\pi\lambda^{-1}\left\{vt - r\cos\theta\right\}\right] \\ &+ \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta - 120^\circ)\right\}\right] \\ &+ \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta + 120^\circ)\right\}\right] \\ = &\cos\left\{2\pi\lambda^{-1}(vt - x)\right\} \\ &+ 2\cos\left\{2\pi\lambda^{-1}\left(vt + \frac{x}{2}\right)\right\}\cos(\pi\lambda^{-1}y\sqrt{3}). \end{aligned}$$

When z is a maximum we must have

$$\cos\left\{2\pi\lambda^{-1}(vt - x)\right\} = 1;$$

and either

$$\cos\left\{2\pi\lambda^{-1}\left(vt+\frac{x}{2}\right)\right\}=1, \text{ and } \cos(\pi\lambda^{-1}\sqrt{3})=1,$$

or

$$\cos\left\{2\pi\lambda^{-1}\left(vt+\frac{x}{2}\right)\right\}=-1, \text{ and } \cos(\pi\lambda^{-1}\sqrt{3})=-1.$$

These conditions are satisfied by the sets of values given in the following table; l, m, n being any integers:—

$\lambda^{-1}vt$	$l-\frac{1}{3}$	$l-\frac{1}{3}$	l	l	$l+\frac{1}{3}$	$l+\frac{1}{3}$
$\lambda^{-1}x$	$2m+\frac{2}{3}$	$2m-\frac{1}{3}$	$2m$	$2m+1$	$2m-\frac{2}{3}$	$2m+\frac{1}{3}$
$\lambda^{-1}y\sqrt{3}$	$2n$	$-2n$	$2n$	$-2n$	$2n$	$-2n$

Let Z be the amplitude of the vibration at any point, then Z is the maximum value of z with respect to t. Obtaining this we get

$$h^{-2}Z^2=\sin^2(\pi\lambda^{-1}3x)+\{\cos(\pi\lambda^{-1}3x)+2\cos(\pi\lambda^{-1}y\sqrt{3})\}^2.$$

At the nodes $Z=0$, and therefore

$$\sin(\pi\lambda^{-1}3x)=0,$$

and

$$\cos(\pi\lambda^{-1}3x)+2\cos(\pi\lambda^{-1}y\sqrt{3})=0.$$

These conditions are satisfied when

$$\lambda^{-1}3x=2m \quad\text{and}\quad \lambda^{-1}y\sqrt{3}=2n\pm\tfrac{1}{3},$$

or

$$\lambda^{-1}3x=2m+1 \text{ and } \lambda^{-1}y\sqrt{3}=2n\pm\tfrac{2}{3}.$$

Putting $y=0$ in the above equations, we get

$$h^{-1}z=\cos\{2\pi\lambda^{-1}(vt-x)\}+2\cos\left\{2\pi\lambda^{-1}\left(vt+\frac{x}{2}\right)\right\},$$
$$h^{-2}Z^2=5+4\cos(\pi\lambda^{-1}3x).$$

The former of these equations gives a section of the film at time t, through a ventral segment in the direction of any one of the waves; and the latter gives a similar section of the surface which encloses the space within which this film vibrates. By putting $x=0$, we get

$$h^{-1}z=\cos(2\pi\lambda^{-1}vt)\{1+2\cos(\pi\lambda^{-1}y\sqrt{3})\},$$
$$h^{-2}Z^2=\{\cos(\pi\lambda^{-1}3x)+2\}^2;$$

and these equations give similar sections to the former ones, but in directions parallel to the fronts of the waves.

The next case we will examine is that of six waves of the same amplitude meeting each other in pairs, the directions of

the pairs being inclined to one another at angles of 120°, with the condition that at some one point all the vibrations shall be in the same phase. These may be divided into two sets of three waves each; and the position of the ventral segments of the first set may be represented as in fig. 2. The position of the ventral segments of the second set may be represented by a similar figure, except that we should have to put 2 instead of 3, and 3 instead of 2, in numbering the ventral segments. In superposing the one figure on the other, we must make a ventral segment of the one figure coincide with a ventral segment of the same numeral of the other. Let a *one* of each figure coincide, then all the *ones* will coincide, and will indicate the points of maximum vibration of the film. On the other numerals the film will not have its maximum vibration, as one set of vibrations will partly destroy the other.

We can get a simple algebraic expression for the form of the film.

Divide the waves into two sets as before, and let z_1 be the displacement due to one set, z_2 that due to the other. Then we have

$$h^{-1}z_1 = \cos\{2\pi\lambda^{-1}(vt-x)\} + 2\cos\left\{2\pi\lambda^{-1}\left(vt+\frac{x}{2}\right)\right\}\cos(\pi\lambda^{-1}y\sqrt{3}).$$

By changing the sign of x and y we turn the whole figure through 180°, and so reverse the motions of the waves; hence we get

$$h^{-1}z_2 = \cos\{2\pi\lambda^{-1}(vt+x)\} + 2\cos\left\{2\pi\lambda^{-1}\left(vt-\frac{x}{2}\right)\right\}\cos(\pi\lambda^{-1}y\sqrt{3});$$

$$\begin{aligned}\therefore\ h^{-1}z &= h^{-1}z_1 + h^{-1}z_2 \\ &= \cos(2\pi\lambda^{-1}vt)\{2\cos(2\pi\lambda^{-1}x) + 4\cos(\pi\lambda^{-1}x)\cos(\pi\lambda^{-1}y\sqrt{3})\},\end{aligned}$$

$$h^{-1}Z = 2\cos(2\pi\lambda^{-1}x) + 4\cos(\pi\lambda^{-1}x)\cos(\pi\lambda^{-1}y\sqrt{3}).$$

The results of this equation are represented in fig. 4. The large dots represent the points at which Z is at its maximum, viz. $6h$. They occur when

$$-\cos\pi\lambda^{-1}x = \cos\pi\lambda^{-1}y = \pm 1;$$

that is, when

$$x = 2m\lambda, \qquad y\sqrt{3} = 2n\lambda,$$

and when

$$x = (2m+1)\lambda, \quad y\sqrt{3} = (2n+1)\lambda.$$

The small dots represent points at which $Z = -3h$. Putting this value for Z, the equation becomes

$$0 = \sin^2(\pi\lambda^{-1}y\sqrt{3}) + \{\cos(\pi\lambda^{-1}y\sqrt{3}) + 2\cos(\pi\lambda^{-1}x)\}^2.$$

This is satisfied when

$$2\cos(\pi\lambda^{-1}x) = -\cos(\pi\lambda^{-1}y\sqrt{3}) = \pm 1;$$

that is, when

$$y\sqrt{3} = 2n\lambda, \qquad x = (2m \pm \tfrac{2}{3})\lambda,$$

and when

$$y\sqrt{3} = (2n+1)\lambda, \quad x = (2m \pm \tfrac{1}{3})\lambda.$$

The dotted lines give the locus of points at which $Z = -2h$. Putting this value into the equation, we get

$$0 = \cos(\pi\lambda^{-1}x)\{\cos(\pi\lambda^{-1}x) + \cos(\pi\lambda^{-1}y\sqrt{3})\}.$$

This equation is satisfied when

$$x = (m + \tfrac{1}{2})\lambda,$$

and when

$$x \pm y\sqrt{3} = (2n+1)\lambda;$$

so that the locus consists of three sets of parallel straight lines.

The nodal lines are obtained by putting $Z = 0$. The equation to them is

$$0 = \cos(2\pi\lambda^{-1}x) + 2\cos(\pi\lambda^{-1}x)\cos(\pi\lambda^{-1}y\sqrt{3}).$$

It is obvious from the loci already obtained, that these lines must be closed curves surrounding the points for which $Z = 6h$; and that they must approximate to an hexagonal form, the greatest radii being towards the corners, and the least perpendicular to the sides of the hexagons formed by the locus of $Z = -2h$.

Putting $y = 0$, we have

$$x = \frac{\lambda}{\pi} . \cos^{-1} . \frac{\sqrt{3}-1}{2} = \cdot 381 . \lambda.$$

Putting $x = 0$, we have

$$y = \frac{2\lambda}{3\sqrt{3}} \qquad = \cdot 385 . \lambda.$$

These are the values of the greatest and least radii; and therefore the nodal lines are very nearly circles, with radii $= \cdot 383 . \lambda$, and centres at the points for which $Z = 6h$. The nodal lines are represented by the circles in fig. 4.

We will next consider the case of four waves meeting two and two, the angle between these directions being 2α, the amplitude of all the waves being the same, with the condition

that at one point all the waves shall be in the same phase. We have

$$h^{-1}z = \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta-\alpha)\right\}\right] + \cos\left[2\pi\lambda^{-1}\left\{vt + r\cos(\theta-\alpha)\right\}\right]$$
$$+ \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta+\alpha)\right\}\right] + \cos\left[2\pi\lambda^{-1}\left\{vt + r\cos(\theta+\alpha)\right\}\right]$$
$$= 4\cos(2\pi\lambda^{-1}vt)\cos(2\pi\lambda^{-1}x\cos\alpha)\cos(2\pi\lambda^{-1}y\sin\alpha).$$

The ventral segments occur when the quantities have the values given in the following table:—

$\lambda^{-1}vt$	l	l	$l+\frac{1}{2}$	$l+\frac{1}{2}$
$\lambda^{-1}x\cos\alpha$	m	$m+\frac{1}{2}$	$m+\frac{1}{2}$	m
$\lambda^{-1}y\sin\alpha$	n	$n+\frac{1}{2}$	n	$n+\frac{1}{2}$

The ventral segments are divided into two sets : all one set vibrate together ; and all the other set vibrate in the opposite direction.

The nodal lines are two sets of straight lines, whose equations are

$$4x\cos\alpha = (2m+1)\lambda$$

and

$$4y\sin\alpha = (2n+1)\lambda.$$

Hence the nodal lines divide the film into oblongs whose length and breadth are

$$\frac{\lambda}{2\cos\alpha} \text{ and } \frac{\lambda}{2\sin\alpha};$$

and a ventral segment lies in the centre of each oblong. When the directions of the waves are $\perp$ each other, $\alpha = 45°$, and the oblongs become squares. This form is shown in fig. 5.

When two waves of equal amplitude meet at an angle $= 2\alpha$, the equation is

$$h^{-1}z = \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta-\alpha)\right\}\right] + \cos\left[2\pi\lambda^{-1}\left\{vt - r\cos(\theta+\alpha)\right\}\right]$$
$$= 2\cos\left\{2\pi\lambda^{-1}(vt - x\cos\alpha)\right\}.\cos(2\pi\lambda^{-1}y\sin\alpha).$$

The nodal lines are the straight lines

$$4y\sin\alpha = (2n+1)\lambda.$$

When $\alpha = 90°$, we get two waves meeting each other in the same direction. The equation becomes

$$h^{-1}z = 2\cos(2\pi\lambda^{-1}vt)\cos(2\pi\lambda^{-1}y).$$

Hitherto we have dealt only with infinite films; we will now consider what forms of vibration can be maintained in films of limited size, the waves undergoing reflection from the boundaries of the film.

When the film is an equilateral triangle, suppose a set of waves to be started having their fronts $\perp$ to one of the sides of the triangle. These waves will be reflected so as to have their fronts $\perp$ to another side, and again reflected so as to have their fronts $\perp$ to the remaining side, and by another reflection they will assume their first direction. If the wave-length is such that the time a wave takes to return to the same position is an integral number of wave-periods, we shall have the case of three sets of equal waves meeting each other at angles of 120°. Fig. 8 shows the form of vibration when the wave-length is $\frac{3}{10}$ of the side of the triangle (no allowance being made for the change of phase at the reflections). The thin lines show the position of the maximum displacement due to each wave at one of the instants at which these lines all pass through certain points, and the numerals 1, 1 show the ventral segments which are then at their maximum displacement, the ventral segments 2, 2 and 3, 3 come to their maximum at different times, as already explained.

Again, suppose two sets of waves to be started in opposite directions, each set having the front $\perp$ to a side of the triangle, and the phases of both sets being the same along a perpendicular from an angle on the opposite side; we shall with a suitable wave-length have six sets of waves meeting each other in pairs, the directions of the pairs making with one another angles of 120°, and all the phases being the same at certain points.

Again, suppose waves to start simultaneously from each of the sides of the equilateral triangle; the waves will be reflected so as to produce other three sets of waves also with their fronts parallel to the sides, by having their direction of motion reversed. Here we again get the six sets of waves above considered, but in a different position. See fig. 6, in which this form of vibration is shown, without allowing for any change of phase at the reflections. The continuous lines show the coincidence of the maximum displacement in one direction due to the waves meeting; and the dotted lines show the same coincidence in the opposite direction. At the black spots we get the coincidence of all the maximum displacements in the same direction; so that these spots show the ventral segments. The wave-length is equal to the distance between the ventral segments $\times \frac{\sqrt{3}}{2}$. With the number of ventral segments shown in the figure the

wave-length equals $\frac{1}{5}$ of the height of the triangle when no allowance is made for change of phase at the reflections. The form of vibration of the film is shown in fig. 4, which has been already explained.

We may obtain a similar set of waves in a rhombus having one of its angles $=60°$, if we suppose sets of waves to start simultaneously from the four sides, and in each direction from the shorter diagonal.

If in such a rhombus a set of waves starts from the longer diagonal, we get the three sets; and if two sets of waves in opposite directions start from the longer diagonal, we get the six sets.

With a right-angled isosceles triangle we may start a set of waves from the hypotenuse, and so get two opposite sets of waves $\parallel$, and two opposite sets $\perp$ to the hypotenuse; and we may get four similar sets of waves in another position by starting waves simultaneously from the two sides of the triangle.

With a square we can get four sets of waves meeting two and two, the directions being $\perp$ to each other, by starting waves simultaneously from all the sides. In fig. 7 the continuous lines show the coincidence of the maximum displacement in one direction of two waves, and the dotted lines show a similar coincidence in the other direction. The black spots show the ventral segments which move together, and the small circles those which move in the opposite directions. The wave-length $=$ the shortest distance between the ventral segments $\times \sqrt{2}$; and with the number of ventral segments shown in the figure, the wave-length, not allowing for change of phase at the reflections, is $\frac{1}{3}$ of the side of the square.

A similar arrangement of waves in another position may be obtained from a square by starting two sets of waves in opposite directions from one of the diagonals.

To obtain four sets of waves meeting each other two and two, the angles between their directions being 2α, take a rectangle having its diagonals inclined at an angle 2α, and start two sets of waves from one of the diagonals; these will by reflection give two sets of waves with fronts parallel to the other diagonal.

With any rectangle two sets of waves meeting each other can be obtained by starting a set of waves from one side of the rectangle.

The case of two sets of waves meeting each other not in the same direction is impossible in a limited film; and I have not been able to discover any form of film which could maintain three sets of waves not making equal angles with one another,

or six sets meeting in pairs whose directions do not make equal angles with one another.

In the phoneidoscope we have a soap-film thrown into a state of vibration by a musical note. The effect is to send the matter of the film towards the ventral segments, and to make them the thickest part of the film. The consequence is that the colours of thin plates are seen less at the ventral segments than at other parts of the film; and we can recognize the ventral segments in this manner. This effect on the film may be illustrated by M. Decharmé's experiments on Chladni's plates*, in which he shows that if a thin layer of water instead of sand be spread over the plate, the water covers the ventral segments and the nodes are left bare.

The two following experiments may, I think, be explained by what has been said.

(1) A square film, $\frac{1}{12}$ of an inch in side, was thrown into vibration by a note having 92 vibrations in a second. The position of the ventral segments was that shown in figs. 5 and 7. Hence the vibration of the film was the result of four sets of waves starting simultaneously from the four sides of the film; and the wave-length was approximately $\frac{1}{3}$ of the side; and the wave-velocity was approximately $\frac{1}{3} \times \frac{11}{12} \times 92$ inches per second—that is, about 28 inches per second.

(2) An equilateral triangle, one inch in height, was thrown into vibration by a note having 152 vibrations in a second. The position of the ventral segments was that shown in figs. 4 and 6. Hence the vibration of the film may be the result of three sets of waves starting simultaneously from the three sides of the Δ, and giving by reflection three other sets moving in the opposite directions. And the wave-length would then be approximately $\frac{1}{5}$ of the height; and the wave-velocity was approximately $\frac{1}{5} \times 1 \times 152$ inches per second, or about $30\frac{1}{2}$ inches per second. Or the figure may be the result of *one* set of waves starting perpendicular to one of the sides of the triangle (see fig. 8). In this case the wave-length would be $\frac{3}{10}$ of the side of the triangle; and the wave-velocity would be $\frac{3}{10} \times \frac{2}{\sqrt{3}} \times 152$ inches per second, or about $52\frac{1}{2}$ inches per second. As this wave-velocity differs very much from the wave-velocity derived from the experiment with the square film, we must reject this latter explanation.

The two experiments may be made to give the same wave-velocity by supposing a change of phase equal to half a period to take place at each reflection. In the first experiment the

* *Ann. de Chim. et de Phys.* sér. 5. vol. xvi. pp. 338–376.

ide of the square has to be taken equal to $\frac{5}{2}$ wave-lengths, and n the second the height of the triangle as equal to $\frac{3}{2}$ wave-engths. The two expressions for the wave-velocity become $\times \frac{11}{12} \times 92$, and $\frac{2}{9} \times 1 \times 152$, both of which expressions are qual to $33\frac{3}{4}$. Hence we are perhaps justified in inferring hat the edges are stationary, and that the wave-velocity in he soap-film is nearly 34 inches in a second.

XIV. *Laws governing the Decomposition of Equivalent Solutions of Iodides under the Influence of Actinism.* By ALBERT R. LEEDS, *Ph.D.**

IN a paper published in the Philosophical Magazine for June 1879, I have given a brief review of the contro-ersy as to whether potassium iodide, in a very dilute solution, s decomposable by sulphuric acid. I likewise pointed out, hat the explanation of the opposite views entertained by ex-erimenters upon this question was due to their having over-ooked the essential part played by air or oxygen in the reac-ion. This last was brought to view by Baumert†, in the ourse of experiments by which he showed that Andrews‡, in e famous investigation undertaken to prove that Baumert's ypothesis that electrolytic ozone is a teroxide of hydrogen§, as false, had himself fallen into an error. For Baumert owed that when a stream of electrolytic ozone has been de-rived of all its active oxygen by passing through a *neutral* lution of iodide of potassium, it may bring about a liberation f iodine in an *acidified* solution, placed later in the series, any times greater (from 4 to 10 in the experiments tried) an that effected by the ozone itself in the first instance. So e curious fact remains, that while Andrews's main conclusion true, all the results by which he succeeded in establishing it e affected by a constant error, and are in excess of their true lues. The triumph of Andrews's opinion (1856) that ozone ntains no hydrogen whatsoever, but in its substance-matter identical with the matter of ordinary oxygen, probably ex-ains why the permanently valuable part of Baumert's work s generally been lost sight of, and why the erroneous me-od of titrating ozone with an acidified solution of potassium dide has been persisted in even down to the present day. en years after the facts above stated were made known by

* Communicated by the Author.

† Pogg. *Ann.* xcix. p. 88.

‡ Proc. Roy. Soc. vii. p. 475; Pogg. *Ann.* xcviii. p. 435.

§ Phil. Mag. vi. p. 51; Pogg. *Ann.* lxxxix. p. 38.

Baumert, they were rediscovered by Payen*, who extended their application to the action of nitric, acetic, oxalic, and other acids upon dilute solutions of potassium iodide, in and out of contact with the air.

In former papers†, a summary of which is given in the Philosophical Magazine (*loc. cit.*), I have shown that the presence of oxygen not merely facilitates, but is absolutely essential to, the occurrence of the reactions in all cases, and whether the reaction occurs in open or closed vessels, in the heat or cold, in darkness or in light. The only exception to this law is the case of an acid like nitric, which under the influence of light (the action of heat alone in the absence of light is being investigated) spontaneously breaks up, and supplies by internal change the essential oxygen. The experiments made to determine the rates of decomposition when various iodides in the presence of different acids were submitted to the influence of magnesium, electric, and solar rays, were made with solutions of known though not of chemically equivalent strength. To supply this defect a new series of experiments were performed immediately after the publication of those cited above, and it is to make known certain remarkable laws of actinochemical change deduced from these latter experiments that the present article is written.

The solutions were of such strength that 1 cubic centim. of each of the acids employed was chemically equivalent to 12·(cubic centims. of a normal caustic soda solution; the iodides were each exactly equivalent to the 20-per-cent. solution of the potassium iodide. 1 cubic centim. of the iodide and 1 cubic centim. of the acid were used in each trial, the volume of the test being made up to 100 cubic centims. by the addition of distilled water. The tests were contained in "comparison-tubes" made of thin perfectly colourless glass, of uniform bore and dimensions, which were supported on frames in such a manner that each tube should be normal to the incident rays, and, in the case of magnesium and electric lamps, 6 inches from the focus of the light.

The first set of trials was made to determine whether the addition of starch as an indicator facilitated the decomposition under the influence of light, as had been originally supposed, or whether, as later on there had been reason to think, it retarded the reaction. In this, as in succeeding experiments, the amounts of iodine liberated are given in milligrams.

* *Comptes Rendus*, lxii. p. 254.

† Proc. Amer. Chem. Soc. 1878, ii. no. 4; Journ. Amer. Chem. Sc 1879, i. p. 18; ibid. p. 65.

Effect of Starch on the Rate of Change (*March* 18, 1879).
Brilliant Sun.

Reagents.	1.30–2.30 P.M. 5 cub. centims. starch-water.	2.30–3.30. 30 cub. centims. starch.	3.30–4.30. No starch.
$H_2SO_4 + KI$ . .	0·48	0·51	1·65
HCl + „ . .	1·18	1·46	3·50
HNO_3 + „ . .	3·10	2·57	3·50
$H_2SO_4 + CdI_2$ . .	0·49	0·82	1·65
HCl + „ . .	1·90	1·11	3·55
HNO_3 + „ . .	2·80	2·83	3·40
$H_2SO_4 + LiI$ . .	0·34	0·40	0·30
HCl + „ . .	0·59	0·72	1·75
HNO_3 + „ . .	2·7	3·86	2·75
$H_2SO_4 + KI$ . .	0·48		

March 19. 12 (noon)–1 P.M. *Feeble Sunlight.*

$H_2SO_4 + KI$ . .	0·26	...	1·8
HCl + „ . .	0·40	...	2·4
$H_2SO_4 + CdI_2$ . .	0·22	...	1·6
HCl + „ . .	0·40	...	2·2
$H_2SO_4 + LiI$ . .	0·20	...	1·2

The experiments of March 18, performed as they were with a sun approaching the horizon, having been less decisive than could have been desired, they were repeated on the following day, but with the disadvantage of a feeble sunlight, with the result of showing that six times more iodine was set free in the absence than in the presence of starch. The suspended precipitate of starch iodide cut off the light, except upon the superficies of the solution. Henceforth the employment of starch was abandoned.

The next set of trials was instituted in order to observe the influence, upon the rate of change, of larger access of oxygen than that derivable from the air already dissolved, or in contact with the solution at its upper surface. To this end tests were prepared in duplicate; and through one pair of these duplicate solutions, the comparison-tubes being connected together in the manner of wash-bottles, a slow current of oxygen was passed.

Influence of increased Supply of Oxygen. March 19, 1879.
11 A.M.–12. *Feeble Sunlight.*

	With oxygen.	Without.
H_2SO_4+KI	7·15	2·9
HCl + „	10·60	4·3

Also with Nitric Acid. 1–2 P.M. *Snowing at time.*

H_2SO_4+KI . . .	5·25	1·5
HNO_3 + „ . . .	6·75	2·5

The absorption of oxygen when sulphuric and hydrochloric acids are employed is expressed by the equations

$$(1)\quad 4KI+2H_2SO_4+O_2=2K_2SO_4+2H_2O+2I_2,$$

and

$$(2)\quad 4KI+4HCl+O_2=4KCl+2H_2O+2I_2.$$

And since 100 cubic centims. of water, when saturated with air under the ordinary circumstances of temperature and pressure, would hold in solution only about 1 mgrm. of oxygen, the maximum amount of iodine which could be liberated during one of these tests, in case no fresh absorption of oxygen took place from the upper surface, would be ·16 mgrm. The influence of these conditions upon the accuracy of the estimations made with the iodo-acid actinometers is being submitted to further investigation.

In the case of nitric acid the reactions become much more complicated. For, in the first place, of the three mineral acids, nitric is the only one which spontaneously decomposes when subjected to sunlight in closed vessels. This is true both of the concentrated acids and when diluted with 500 times their volume of water. Moreover, in the latter case the presence of starch had no influence except in the nitric acid, in which it nearly doubled the rate of decomposition. The same effect of starch (whether it is true of organic matter in general has not been determined) is to be noted in the above table of decompositions for March 18, in which it will be seen that in the trials where starch was present the amounts of iodine liberated by nitric acid were largely in excess of those set free by equivalent amounts of other acids. In fine, while nitric acid conforms to the general law of actinic change, as expressed by the equation

$$4KI+4HNO_3+O_2=4KNO_3+2H_2O+2I_2,$$

it is likewise subject to the special decomposition

$$4HNO_3 \text{ (in sunlight)} =2N_2O_4+2H_2O+O_2.$$

For these reasons, at an early stage of the inquiry, the actino

ıetric use of nitric acid was discontinued until the exact influnce of temperature, actinism, oxygen, and organic matter ıpon its special rate of change had been established, and the ıature of the accompanying reactions*.

In order to study the influence of mineral acids, trials were ıade as above, the solutions being of such strength that 1 c.c. of ach was chemically equivalent to the same amount of mineral cid.

Effect of Organic Acids. March 20. 10.10 A.M. to 1.10 P.M.
Good Sunlight.

Oxalic acid	+ potassium iodide	= 6·5 mgrms. I.	
"	+ cadmium "	= 6·0	"
"	+ lithium "	= 5·75	"
Tartaric acid	+ potassium "	= 0·10 mgrm. I.	
"	+ cadmium "	= 0·00	"
"	+ lithium "	= 0·00	"

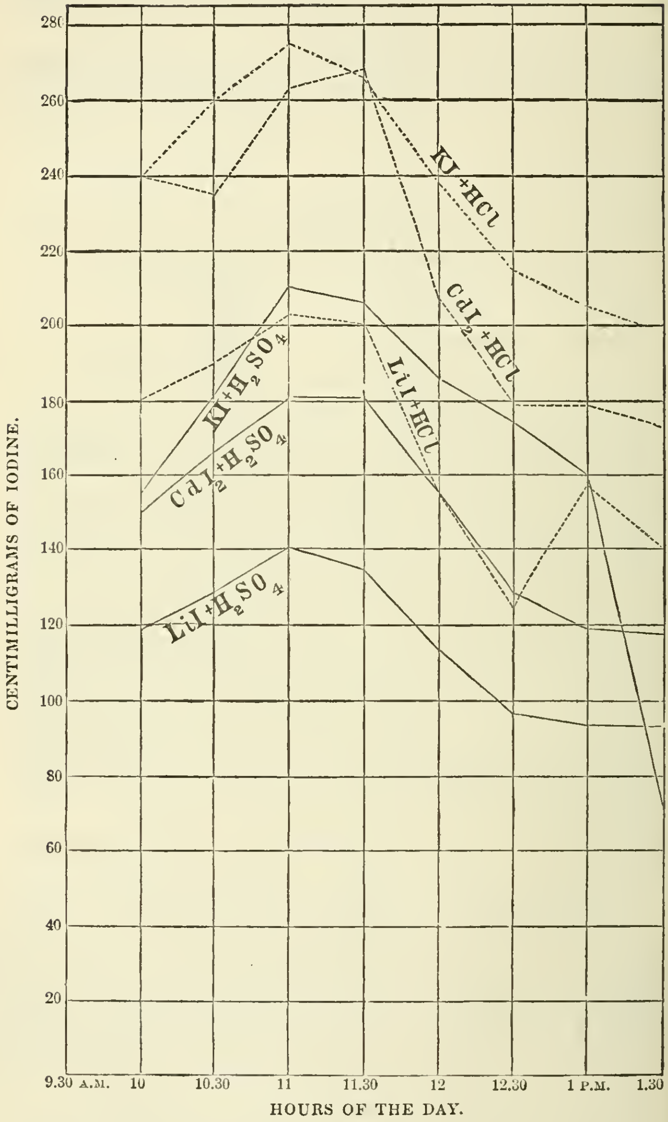
Nor any change with acetic acid during this three-hour nterval. To determine more precisely the rate of change ffected by the organic as compared with the mineral acids, it vill be necessary to make simultaneous estimations; but even vith oxalic acid, the most active of them all, it is evident that he amount of decomposition was relatively small.

An actinometric measurement of the solar ray was made vith the equivalent solutions of various iodides and acids—the ɪrincipal object being to note the effect upon the rate of decomposition of different acids in the presence of the same base, nd of different basic radicals of the iodides in the presence of he same acid. The sunlight, which was good during the arly part of the day, declined towards noon ; and shortly fterwards the sky became overcast.

Actinometric Measurement of the Solar Ray. March 26, 1879.

	9.30–10 A.M.	10–10.30.	10.30–11.	11–11.30.	11.30–12.	12–12.30.	12.30–1.	1–1.30.	Means.	Ratios.
O_4+KI...	1·55	1·81	2·1	2·06	1·87	1·7	1·6	0·75	1·68	H_2SO_4 : HCl.
+KI...	2·4	2·6	2·75	2·66	2·39	2·15	2·05	2·0	2·5	1 : 1·48
O_4+CdI..	1·5	1·62	1·81	1·81	1·56	1·29	1·19	1·18	1·49	H_2SO_4 : HCl.
+CdI .	2·4	2·35	2·63	2·68	2·09	1·79	1·79	1·75	2·18	1 : 1·46
O_4+LiI ..	1·18	1·28	1·41	1·35	1·13	0·96	0·93	0·93	1·15	H_2SO_4 : HCl.
+LiI ..	1·83	1·9	2·03	2·0	1·56	1·25	1·58	1·4	1·69	1 : 1·47

* Gay-Lussac states (*Ann. de Chim. et de Phys.* 1816, p. 317) that no ecomposition of dilute acid takes place in the light, except in the prence of a certain quantity of concentrated sulphuric acid ; also that the ecomposition is into nitrous acid.


CENTIMILLIGRAMS OF IODINE.
280
260
240
220
200
180
160
140
120
100
80
60
40
20
9.30 A.M.
10
10.30
11
11.30
12
12.30
1 P.M.
1.30
HOURS OF THE DAY.
KI+HCl
CdI2+HCl
LiI+HCl
KI+H2SO4
CdI2+H2SO4
LiI+H2SO4

On comparison of the arithmetical means of the results obtained in fifty-four trials during the course of the same day, the striking fact is brought out that the amounts of iodine liberated by the two acids in the presence of the same base stand in a constant ratio to one another. The law of actinic force herein indicated may provisionally be expressed by the formula—The chemism of the chlorine radical is to that of the $\frac{1}{2}SO_4$ radical (measured by the relative amounts of iodine liberated by each respectively in solutions of the metallic iodides exposed to the sunlight) as 1·47 : 1. There is no reason for inferring from the experiments that a similar definite ratio exists between the amounts of iodine liberated from different soluble iodides in the presence of the same acid.

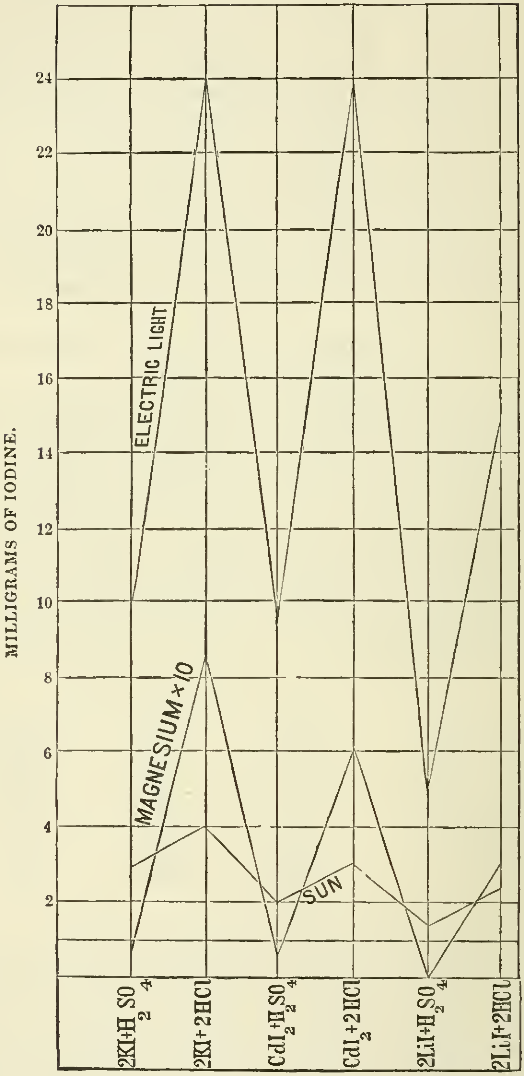
Comparison of the Actinic Intensities of the Solar, Electric, and Magnesium Light.

In making this comparison, the amounts of iodine liberated at the hour of maximum actinic intensity (1–1.30 P.M.) were taken in the case of the sun. The electric light was that emanating from a lamp of 7000 candle-power falling upon the solutions at a distance of six inches; the magnesium light, that derived from the burning of a single ribbon in the ordinary lamp placed at the same distance. The time of exposure to the electric light was 10 minutes, to the magnesium light 25 minutes, to the sun $\frac{1}{2}$ an hour; in the table all are calculated to one hour.

	Solar.	Electric.	Magnesium.
$H_2SO_4 + KI$	2·70	10·0	0·084
HCl + „	4·00	24·0	0·87
$H_2SO_4 + CdI_2$ . .	2·04	9·5	0·072
HCl + „	3·00	24·0	0·6
$H_2SO_4 + LiI$	1·44	5·0	none
HCl + „	2·40	15·0	0·3

On examining this table, and still more readily the graphical illustration accompanying it, two phenomena become forcibly manifest:—

1st. The very much greater actinic intensity of the electric as compared with the solar ray, when compared in the manner indicated, and the very much less intensity of the magnesium. In order to bring the magnesium curve into the same diagram as the others, the numbers in the magnesium column were all multiplied by 10.


MILLIGRAMS OF IODINE.
24
22
20
18
16
14
12
10
8
6
4
2
ELECTRIC LIGHT
MAGNESIUM × 10
SUN
2KI+H2SO4
2KI+2HCl
CdI2+H2SO4
CdI2+2HCl
2LiI+H2SO4
2LiI+2HCl

2nd. Instead of the relative chemism of the chlorine atom, as estimated by its iodine-liberating power under these conditions, being 1·5 in all three cases, it is 6 with the electric and 10 with the magnesium light. The examination of these differences, as related to the actinic forces of different sources of light and to different acid and basic radicals, is being investigated further.

Finally, the influence of absorbing media upon the invisible rays of the sun and electric light were determined—the comparison-tubes being surrounded by a thickness of 3 cubic metres of ammonio-sulphate of copper, neutral potassium, chromate, and fuchsine, each solution being brought to the apparent degree of translucency for the blue, yellow, and red respectively. The exposure to the sun was from 9.30 A.M. to 5·30 P.M., to the electric light 20 minutes; but both are reduced to the interval of 1 hour.

		Sun.	Electric light.
Blue.	H_2SO_4 + KI . .	0·74	1·80
	HCl + „ . .	1·12	6·75
Yellow.	H_2SO_4 + KI . .	0·11	0·00
	HCl + „ . .	0·25	0·125
Red.	H_2SO_4 + KI . .	0·28	0·60
	HCl + „ . .	0·56	2·25

These figures show that the selective action of absorbing media upon the invisible rays of different illuminants varies greatly, and suggests the employment of this method for the mapping out of absorption-spectra for the actinic portion.

Stevens Institute of Technology,
June 1880.

XV. *Vortex Statics. By* Sir William Thomson*.

THE subject of this paper is *steady motion* of vortices.

1. Extended definition of "steady motion." The motion of any system of solid or fluid or solid and fluid matter is said to be steady when its configuration remains equal and similar, and the velocities of homologous particles equal, however the configuration may move in space, and however distant individual material particles may at one time be from the points homologous to their positions at another time.

2. Examples of steady and not steady motion:—

(1) A rigid body symmetrical round an axis, set to rotate round any axis through its centre of gravity, and left free,

* From the Proceedings of the Royal Society of Edinburgh, Session 1875–76. Communicated by the Author.

performs steady motion. Not so a body having three unequal principal moments of inertia.

(2) A rigid body of any shape, in an infinite homogeneous liquid, rotating uniformly round any, always the same, fixed line, and moving uniformly parallel to this line, is a case of steady motion.

(3) A perforated rigid body in an infinite liquid moving in the manner of example (2), and having cyclic irrotational motion of the liquid through its perforations, is a case of steady motion. To this case belongs the irrotational motion of liquid in the neighbourhood of any rotationally moving portion of fluid of the same shape as the solid, provided the distribution of the rotational motion is such that the shape of the portion endowed with it remains unchanged. The object of the present paper is to investigate general conditions for the fulfilment of this proviso, and to investigate, further, the conditions of stability of distribution of vortex motion satisfying the condition of steadiness.

3. *General Synthetical Condition for Steadiness of Vortex Motion.*—The change of the fluid's molecular rotation at any point fixed in space must be the same as if for the rotationally moving portion of the fluid were substituted a solid, with the amount and direction of axis of the fluid's actual molecular rotation inscribed or marked at every point of it, and the whole solid, carrying these inscriptions with it, were compelled to move in some manner answering to the description of example (2). If at any instant the distribution of molecular rotation* through the fluid, and corresponding distribution of fluid-velocity, are such as to fulfil this condition, it will be fulfilled through all time.

4. *General Analytical Condition for Steadiness of Vortex Motion.*—If, with (§ 24, below) vorticity and "impulse" given, the kinetic energy is a maximum or a minimum, it is obvious that the motion is not only steady, but stable. If, with same conditions, the energy is a maximum-minimum, the motion is clearly steady, but it may be either unstable or stable.

5. The simple circular Helmholtz ring is a case of stable steady motion, with energy maximum-minimum for given vorticity and given impulse. A circular vortex ring, with an inner irrotational annular core, surrounded by a rotationally moving annular shell (or endless tube), with irrotational cir-

* One of Helmholtz's now well-known fundamental theorems shows that, *from the molecular rotation at every point of an infinite fluid*, the velocity at every point is determinate, being expressed synthetically by the same formulæ as those for finding the "magnetic resultant force" of a pure electromagnet. (Thomson's Reprint of Papers on Electrostatics and Magnetism.)

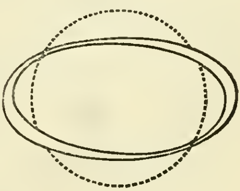
culation outside all, is a case of motion which is steady, if the outer and inner contours of the section of the rotational shell are properly shaped, but certainly unstable if the shell be too thin. In this case also the energy is maximum-minimum for circular given vorticity and given impulse.

6. In these examples of steady motion, the "resultant impulse" (V. M.* § 8) is a simple impulsive force, without couple: the corresponding rigid body of example (3) is a toroid; and its motion is purely translational and parallel to the axis of the toroid.

We have also exceedingly interesting cases of steady motion in which the impulse is such that, if applied to a rigid body, it would be reducible, according to Poinsot's method, to an impulsive force in a determinate line, *and a couple with this line for axis.* To this category belong certain distributions of vorticity giving longitudinal vibrations, with thickenings and thinnings of the core travelling as waves in one direction or the other round a vortex-ring, which will be investigated in a future communication to the Royal Society. In all such cases the corresponding rigid body of § 2 example (2) has both rotational and translational motion.

7. To find illustrations, suppose, first, the vorticity (defined below, § 24) and the force resultant of the impulse to be (according to the conditions explained below, § 29) such that the cross section is small in comparison with the aperture. Take a ring of flexible wire (a piece of very stout lead wire with its ends soldered together answers well), bend it into an oval form, and then give it a right-handed twist round the long axis of the oval, so that the curve comes to be not in one plane (fig. 1). A properly-shaped twisted ellipse of this kind [a shape perfectly determinate when the vorticity, the force resultant of the impulse, and the rotational moment of the impulse (V. M. § 6), are all given] is the figure of the core in what we may call the first† steady mode of single and simple toroidal vortex motion with rotational moment. To illustrate the second steady mode, commence with a circular ring of flexible wire, and pull it out,

Fig. 1.



* My first series of papers on Vortex Motion in the Transactions of the Royal Society of Edinburgh will be thus referred to henceforth.

† First or greatest, and second, and third, and higher modes of steady motion to be regarded as analogous to the first, second, third, and higher fundamental modes of an elastic vibrator, or of a stretched cord, or of steady undulatory motion in an endless uniform canal, or in an endless chain of mutually repulsive links.

at three points 120° from one another, so as to make it into as it were an equilateral triangle with rounded corners. Give now a right-handed twist, round the radius to each corner, to the plane of the curve at and near the corner; and, keeping the character of the twist thus given to the wire, bend it into a certain determinate shape proper for the data of the vortex motion. This is the shape of the vortex core in the second steady mode of single and simple toroidal vortex motion with rotational moment. The third is to be similarly arrived at, by twisting the corners of a square having rounded corners; the fourth, by twisting the corners of a regular pentagon having rounded corners; the fifth, by twisting the corners of a hexagon, and so on.

In each of the annexed diagrams of toroidal helices a circle is introduced to guide the judgment as to the relief above and depression below the plane of the diagram which the curve represented in each case must be imagined to have. The circle may be imagined in each case to be the circular axis of a toroidal core on which the helix may be supposed to be wound.

To avoid circumlocution, I have said "give a right-handed twist" in each case. The result in each case, as in fig. 1, illustrates a vortex motion for which the corresponding rigid body describes left-handed helices, by all its particles, round the central axis of the motion. If now, instead of right-handed twists to the plane of the oval, or the corners of the triangle, square, pentagon, &c., we give left-handed twists, as in figs. 2, 3, 4, the result in each case will be a vortex motion

Fig. 2.

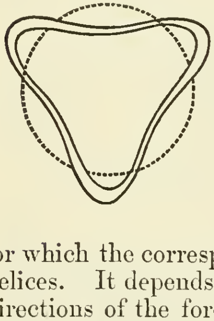


Fig. 3.

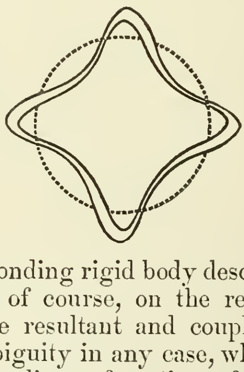
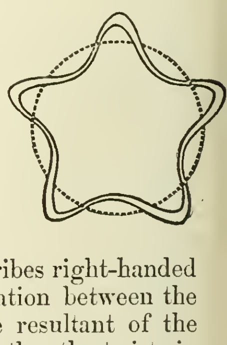


Fig. 4.



for which the corresponding rigid body describes right-handed helices. It depends, of course, on the relation between the directions of the force resultant and couple resultant of the impulse, with no ambiguity in any case, whether the twists in the forms, and in the lines of motion of the corresponding rigid body, will be right-handed or left-handed.

8. In each of these modes of motion the energy is a maximum-minimum for given force resultant and given couple

resultant of impulse. The modes successively described above are successive solutions of the maximum-minimum problem of § 4—a determinate problem with the multiple solutions indicated above, but no other solution, when the vorticity is given in a single simple ring of the liquid.

9. The problem of steady motion, for the case of a vortex-line with infinitely thin core, bears a close analogy to the following purely geometrical problem:—

Find the curve whose length shall be a minimum with given resultant projectional area, and given resultant areal moment (§ 27 below). This would be identical with the vortex problem if the energy of an infinitely thin vortex ring of given volume and given cyclic constant were a function simply of its apertural circumference. The geometrical problem clearly has multiple solutions answering precisely to the solutions of the vortex problem.

10. The very high modes of solution are clearly very nearly identical for the two problems (infinitely high modes identical), and are found thus:—

Take the solution derived in the manner explained above, from a regular polygon of N sides, when N is a very great number. It is obvious that either problem must lead to a form of curve like that of a long regular spiral spring of the ordinary kind bent round till its two ends meet, and then having its ends properly cut and joined so as to give a continuous endless helix with axis a circle (instead of the ordinary straight line-axis), and N turns of the spiral round its circular axis. This curve I call a toroidal helix, because it lies on a toroid*, just as the common regular helix lies on a circular cylinder.

* I call a circular toroid a simple ring generated by the revolution of any singly-circumferential closed plane curve round any axis in its plane not cutting it. A "tore," following French usage, is a ring generated by the revolution of a circle round any line in its plane not cutting it. Any simple ring, or any solid with a single hole through it, may be called a toroid; but to deserve this appellation it had better be not very unlike a tore.

The endless closed axis of a toroid is a line through its substance passing somewhat approximately through the centres of gravity of all its cross sections. An apertural circumference of a toroid is any closed line in its surface once round its aperture. An apertural section of a toroid is any section by a plane or curved surface which would cut the toroid into two separate toroids. It must cut the surface of the toroid in just two simple closed curves, one of them completely surrounding the other on the sectional surface: of course it is the space between these curves which is the actual section of the toroidal substance; and the area of the inner one of the two is a section of the aperture.

A section by any surface cutting every apertural circumference, each once and only once, is called a cross section of the toroid. It consists essentially of a simple closed curve.

Let a be the radius of the circle thus formed by the axis of the closed helix; let r denote the radius of the cross section of the ideal toroid on the surface of which the helix lies, supposed small in comparison with a; and let θ denote the inclination of the helix to the normal section of the toroid. We have

$$\tan\theta = \frac{2\pi a}{\mathrm{N} \,.\, 2\pi r} = \frac{a}{\mathrm{N}r},$$

because $\frac{2\pi a}{\mathrm{N}}$ is, as it were, the step of the screw, and $2\pi r$ is the circumference of the cylindrical core on which any short part of it may be approximately supposed to be wound.

Let κ be the cyclic constant, I the given force resultant of the impulse, and μ the given rotational moment. We have (§ 28) approximately

$$\mathrm{I} = \kappa\pi a^2, \quad \mu = \kappa \mathrm{N}\pi r^2 a.$$

Hence

$$a = \sqrt{\frac{\mathrm{I}}{\kappa\pi}}, \quad r = \sqrt{\frac{\mu}{\mathrm{N}\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}\mathrm{I}^{\frac{1}{2}}}},$$

$$\tan\theta = \sqrt{\frac{\mathrm{I}^{\frac{3}{2}}}{\mathrm{N}\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

11. Suppose now, instead of a single thread wound spirally round a toroidal core, we have two separate threads forming, as it were, a "two-threaded screw," and let each thread make a whole number of turns round the toroidal core. The two threads, each endless, will be two helically tortuous rings linked together, and will constitute the core of what will now be a double vortex-ring. The formulæ just now obtained for a single thread would be applicable to each thread, if κ denoted the cyclic constant for the circuit round the two threads, or twice the cyclic constant for either, and N the number of turns of either alone round the toroidal core. But it is more convenient to take N for the number of turns of both threads (so that the number of turns of one thread alone is $\frac{1}{2}$N), and κ the cyclic constant for either thread alone, and thus for very high steady modes of the double vortex ring,

$$\mathrm{I} = 2\kappa\pi a^2, \quad \mu = \kappa\mathrm{N}\pi r^2 a,$$

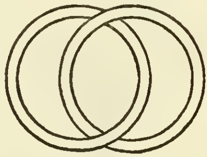
$$\tan\theta = \sqrt{\frac{(\frac{1}{2}\mathrm{I})^{\frac{3}{2}}}{\mathrm{N}\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

Lower and lower steady modes will correspond to smaller and smaller values of N; but in this case, as in the case of the single vortex-core, the form will be a curve of some ultra-transcendent character, except for very great values of N, or

for values of θ infinitely nearly equal to a right angle (this latter limitation leading to the case of infinitely small transverse vibrations).

12. The gravest steady mode of the double vortex-ring corresponds to $N=2$. This with the single vortex-core gives the case of the twisted ellipse (§ 7). With the double core it gives a system which is most easily understood by taking two plane circular rings of stiff metal linked together. First, place them as nearly coincident as their being linked together permits (fig. 5). Then separate them a little, and incline their planes a little, as shown in the diagram. Then bend each into an unknown shape determined by the strict solution of the transcendental problem of analysis to which the hydro-kinetic investigation leads for this case.

Fig. 5.



13. Go back now to the supposition of § 11, and alter it to this:—

Let each thread make one turn and a half, or any odd number of half turns, round the toroidal core: thus each thread will have an end coincident with an end of the other. Let these coincident ends be united. Thus there will be but one endless thread making an odd number N of turns round the toroidal core. The cases of $N=3$ and $N=9$ are represented in the annexed diagrams (figs. 8 and 9)*.

Imagine now a three-threaded toroidal helix, and let N denote the whole number of turns round the toroidal core; we have

$$I=3\kappa\pi a^2, \quad \mu=\kappa N\pi r^2 a,$$

$$\tan\theta=\sqrt{\frac{(\frac{1}{3}I)^{\frac{3}{2}}}{N\mu\kappa^{\frac{1}{2}}\pi^{\frac{1}{2}}}}.$$

Suppose now N to be divisible by 3; then the three threads form three separate endless rings linked together. The case of $N=3$ is illustrated by the annexed diagram (fig. 6), which is repeated from the diagram of V. M. § 58. If N be not divisible by 3, the three heads run together into one, as illustrated for the case of $N = 14$ in the annexed diagram (fig. 7).

* The first of these was given in § 58 of my paper on Vortex Motion. It has since become known far and wide by being seen on the back of the "Unseen Universe.

Fig. 6.

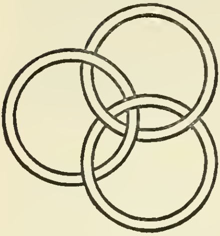


Fig. 7.

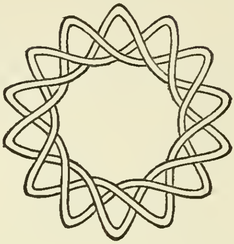
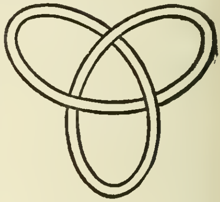


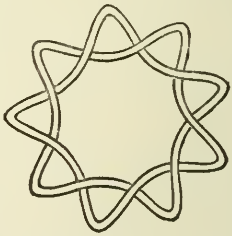
Fig. 8. "Trefoil Knot."



14. The irrotational motion of the liquid round the rotational cores in all these cases is such that the fluid-velocity at any point is equal to, and in the same direction as, the resultant magnetic force at the corresponding point in the neighbourhood of a closed galvanic circuit, or galvanic circuits, of the same shape as the core or cores. The setting-forth of this analogy to people familiar, as modern naturalists are, with the distribution of magnetic force in the neighbourhood of an electric circuit, does much to promote a clear understanding of the still somewhat strange fluid-motions with which we are at present occupied.

15. To understand the motion of the liquid in the rotational core itself, take a piece of Indian-rubber gas-pipe stiffened internally with wire in the usual manner, and with it construct any of the forms with which we have been occupied, for instance the symmetrical trefoil knot (fig. 8, § 13), uniting the two ends of the tube carefully, by tying them firmly, by an inch or two of straight cylindrical plug; then turn the tube round and round, round its sinuous axis. The rotational motion of the fluid vortex-core is thus represented. But it must be remembered that the outer form of the core has a motion perpendicular to the plane of the diagram, and a rotation round an axis through the centre of the diagram and perpendicular to the plane, in each of the cases represented by the preceding diagrams. The whole motion of the fluid, rotational and irrotational, is so related in its different parts to one another, and to the translational and rotational motion of the shape of the core, as to be everywhere slipless.

Fig. 9. "Nine-leaved Knot."



16. Look to the preceding diagrams, and, thinking of what they represent, it is easy to see that there must be a determi-

nate particular shape for each of them which will give steady motion; and I think we may confidently judge that the motion is stable in each, provided only the core is sufficiently thin. It is more easy to judge of the cases in which there are multiple sinuosities by a synthetic view of them (§ 3) than by consideration of the maximum-minimum problem of § 8.

17. It seems probable that the two- or three- or multiple-threaded toroidal helix motions cannot be stable, or even steady, unless I, μ, and N are such as to make the shortest distances between different positions of the core or cores considerable in comparison with the core's diameter. Consider, for example, the simplest case (§ 12, fig. 5) of two simple rings linked together.

18. Go back now to the simple circular Helmholtz ring. It is clear that there must be a shape of absolute maximum energy for given vorticity and given impulse, if we introduce the restriction that the figure is to be a figure of revolution—that is to say, symmetrical round a straight axis. If the given vorticity be given in this determinate shape, the motion will be steady; and there is no other figure of revolution for which it would be steady (it being understood that the impulse has a single force resultant without couple). If the given impulse, divided by the cyclic constant, be very great in comparison with the two-thirds power of the volume of liquid in which the vorticity is given, the figure of steadiness is an exceedingly thin circular ring of large aperture and of approximately circular cross section. This is the case to which chiefly attention is directed by Helmholtz. If, on the other hand, the impulse divided by the cyclic constant be very small compared with the two-thirds power of the volume, the figure becomes like a long oval bored through along its axis of revolution and with the ends of the bore rounded off (or trumpeted) symmetrically, so as to give a figure something like the handle of a child's skipping-rope, but symmetrical on the two sides of the plane through its middle perpendicular to its length. It is certain that, however small the impulse, with given vorticity the figure of steadiness thus indicated is possible, however long in the direction of the axis and small in diameter perpendicular to the axis and in aperture it may be. I cannot, however, say at present that it is certain that this possible steady motion is stable; for there are figures not of revolution, deviating infinitely little from it, in which, with the same vorticity, there is the same impulse and the same energy, and consideration of the general character of the motion is not reassuring on the point of stability when rigorous demonstration is wanting.

19. Hitherto I have not indeed succeeded in rigorously

demonstrating the stability of the Helmholtz ring in any case. With given vorticity, imagine the ring to be thicker in one place than in another. Imagine the given vorticity, instead of being distributed in a symmetrical circular ring, to be distributed in a ring still with a circular axis, but thinner in one part than in the rest. It is clear that, with the same vorticity and the same impulse, the energy with such a distribution is greater than when the ring is symmetrical. But now let the figure of the cross section of the ring, instead of being approximately circular, be made considerably oval. This will diminish the energy with the same vorticity and the same impulse. Thus from the figure of steadiness we may pass continuously to others with same vorticity, same impulse, and same energy. Thus, we see that the figure of steadiness is, as stated above, a figure of maximum-minimum, and not of absolute maximum, nor of absolute minimum energy. Hence, from the maximum-minimum problem we cannot derive proof of stability.

20. The known phenomena of steam-rings and smoke-rings show us enough of, as it were, the natural history of the subject to convince us beforehand that the steady configuration, with ordinary proportions of diameters of core to diameter of aperture, is stable; and considerations connected with what is rigorously demonstrable in respect to stability of vortex columns (to be given in a later communication to the Royal Society) may lead to a rigorous demonstration of stability for a simple Helmholtz ring, if of thin-enough core in proportion to diameter of aperture. But at present neither natural history nor mathematics gives us perfect assurance of stability when the cross section is considerable in proportion to the area of aperture.

21. I conclude with a brief statement of general propositions, definitions, and principles used in the preceding abstract, of which some appeared in my series of papers on vortex motion communicated to the Royal Society of Edinburgh in 1867, –68 and –69, and published in the Transactions for 1869. The rest will form part of the subject of a continuation of that paper, which I hope to communicate to the Royal Society before the end of the present session.

Any portion of a liquid having vortex motion is called *vortex-core*, or, for brevity, simply "core." Any finite portion of liquid which is all vortex-core, and has contiguous with it over its whole boundary irrotationally moving liquid, is called *a vortex*. A vortex thus defined is essentially a ring of matter. That it must be so was first discovered and published by Helmholtz. Sometimes the word *vortex* is extended to include irrotationally moving liquid circulating round or moving

in the neighbourhood of vortex-core; but as different portions of liquid may successively come into the neighbourhood of the core, and pass away again, while the core always remains essentially of the same substance, it is more proper to limit the substantive term *a vortex* as in the definition I have given.

22. *Definition I.*—The circulation of a vortex is the circulation [V. M. § 60 (a)] in any endless circuit once round its core. Whatever varied configurations a vortex may take, whether on account of its own unsteadiness (§ 1 above), or on account of disturbances by other vortices, or by solids immersed in the liquid, or by the solid boundary of the liquid (if the liquid is not infinite), its "circulation" remains unchanged [V. M. § 59, Prop. (1)]. The circulation of a vortex is sometimes called its *cyclic constant.*

Definition II.—An axial line through a fluid moving rotationally, is a line (straight or curved) whose direction at every point coincides with the axis of molecular rotation through that point [V. M. § 59 (2)].

Every axial line in a vortex is essentially a closed curve, being of course wholly without a vortex.

23. *Definition III.*—A closed section of a vortex is any section of its core cutting normally the axial line through every point of it. Divide any closed section of a vortex into smaller areas; the axial lines through the borders of these areas form what are called vortex-tubes. I shall call (after Helmholtz) a vortex-filament any portion of a vortex bounded by a vortex-tube (not necessarily infinitesimal). Of course a complete vortex may be called therefore a vortex-filament; but it is generally convenient to apply this term only to a part of a vortex as just now defined. The boundary of a complete vortex satisfies the definition of a vortex-tube.

A complete vortex-tube is essentially endless. In a vortex-filament infinitely small in all diameters of cross sections "rotation" varies [V. M. § 60 (e)] from point to point of the length of the filament, and from time to time, inversely as the area of the cross section. The product of the area of the cross section into the rotation is equal to the circulation or cyclic constant of the filament.

24. Vorticity will be used to designate in a general way the distribution of molecular rotation in the matter of a vortex. Thus, if we imagine a vortex divided into a number of infinitely thin vortex-filaments, the vorticity will be completely given when the volume of each filament and its circulation, or cyclic constant, are given; but the shapes and positions of the filaments must also be given, in order that not only the vorticity, but its distribution, can be regarded as given.

25. The vortex-density at any point of a vortex is the circulation of an infinitesimal filament through this point, divided by the volume of the complete filament. The vortex-density remains always unchanged for the same portion of fluid. By definition it is the same all along any one vortex-filament.

26. Divide a vortex into infinitesimal filaments inversely as their densities, so that their circulations are equal; and let the circulation of each be $\frac{1}{n}$ of unity. Take the projection of all the filaments on one plane. $\frac{1}{n}$ of the sum of the areas of these projections is (V. M. §§ 6, 62) equal to the component impulse of the vortex perpendicular to that plane. Take the projections of the filaments on three planes at right angles to one another, and find the centre of gravity of the areas of these three sets of projections. Find, according to Poinsot's method, the resultant axis, force, and couple of the three forces equal respectively to $\frac{1}{n}$ of the sums of the areas, and acting in lines through the three centres of gravity perpendicular to the three planes. This will be the resultant axis; the force resultant of the impulse, and the couple resultant of the vortex.

The last of these (that is to say, the couple) is also called the rotational moment of the vortex (V. M. § 6).

27. *Definition* IV.—The moment of a plane area round any axis is the product of the area multiplied into the distance from that axis of the perpendicular to its plane through its centre of gravity.

Definition V.—The area of the projection of a closed curve on the plane for which the area of projection is a maximum will be called the area of projection of the curve, or simply the area of the curve. The area of the projection on any plane perpendicular to the plane of the resultant area is of course zero.

Definition VI.—The resultant axis of a closed curve is a line through the centre of gravity, and perpendicular to the plane of its resultant area. The resultant areal moment of a closed curve is the moment round the resultant axis of the areas of its projections on two planes at right angles to one another, and parallel to this axis. It is understood, of course, that the areas of the projections on these two planes are not evanescent generally, except for the case of a plane curve, and that their zero-values are generally the sums of equal positive and negative portions. Thus their moments are not in general zero.

Thus, according to these definitions, the resultant impulse of a vortex-filament of infinitely small cross section and of unit circulation is equal to the resultant area of its curve. The resultant axis of a vortex is the same as the resultant axis of the curve; and the rotational moment is equal to the resultant areal moment of the curve.

28. Consider for a moment a vortex-filament in an infinite liquid with no disturbing influence of other vortices, or of solids immersed in the liquid. We now see, from the constancy of the impulse (proved generally in V. M. § 19), that the resultant area, and the resultant areal moment of the curve formed by the filament, remain constant however its curve may become contorted; and its resultant axis remains the same line in space. Hence, whatever motions and contortions the vortex-filament may experience, if it has any motion of translation through space this motion must be on the average along the resultant axis.

29. Consider now the actual vortex made up of an infinite number of infinitely small vortex-filaments. If these be of volumes inversely proportional to their vortex-densities (§ 25), so that their circulations are equal, we now see from the constancy of the impulse that the sum of the resultant areas of all the vortex-filaments remains constant; and so does the sum of their rotational moments: and the resultant areal axis of them all regarded as one system is a fixed line in space. Hence, as in the case of a vortex-filament, the translation, if any, through space is on the average along its resultant axis. All this, of course, is on the supposition that there is no other vortex, and no solid immersed in the liquid, and no bounding surface of the liquid near enough to produce any sensible influence on the given vortex.

XVI. *On Gravitational Oscillations of Rotating Water.* *By* Sir WILLIAM THOMSON.*

THIS is really Laplace's subject in his Dynamical Theory of the Tides; where it is dealt with in its utmost generality except one important restriction—the motion of each particle to be infinitely nearly horizontal, and the velocity to be always equal for all particles in the same vertical. This implies that the greatest depth must be small in comparison with the distance that has to be travelled to find the deviation from levelness of the water-surface altered by a sensible fraction of its maximum amount. In the present short communication I

* From the Proceedings of the Royal Society of Edinburgh, March 17, 1879. Communicated by the Author.

adopt this restriction; and, further, instead of supposing the water to cover the whole or a large part of the surface of a solid spheroid as does Laplace, I take the simpler problem of an area of water so small that the equilibrium-figure of its surface is not sensibly curved. Imagine a basin of water of any shape, and of depth not necessarily uniform, but, at greatest, small in comparison with the least diameter. Let this basin and the water in it rotate round a vertical axis with angular velocity ω so small that the greatest equilibrium-slope due to it may be a small fraction of the radian: in other words, the angular velocity must be small in comparison with $\sqrt{\frac{g}{\frac{1}{2}\mathrm{A}}}$, where g denotes gravity, and A the greatest diameter of the basin. The equations of motion are

$$\left.\begin{aligned} \frac{du}{dt}-2\omega v &= -\frac{1}{\rho}\frac{dp}{dx}, \\ \frac{dv}{dt}+2\omega u &= -\frac{1}{\rho}\frac{dp}{dy}; \end{aligned}\right\} \quad \ldots \ldots \quad (1)$$

where u and v are the component velocities of any point of the fluid in the vertical column through the point (xy), relatively to horizontal axes Ox, Oy revolving with the basin; p the pressure at any point x, y, z of this column; and ρ the uniform density of the liquid. The terms $\omega^2 x$, $\omega^2 y$, which appear in ordinary dynamical equations referred to rotating axes, represent components of centrifugal force, and therefore do not appear in these equations. Let now D be the mean depth and $\mathrm{D}+h$ the actual depth at any time t in the position (xy). The "equation of continuity" is

$$\frac{d(\mathrm{D}u)}{dx}+\frac{d(\mathrm{D}v)}{dy}=-\frac{dh}{dt}. \quad \ldots \ldots \quad (2)$$

Lastly, by the condition that the pressure at the free surface is constant, and that the difference of pressures at any two points in the fluid is equal to $g\times$ difference of levels, we have

$$\left.\begin{aligned} \frac{dp}{dx} &= g\rho\frac{dh}{dx}, \\ \frac{dp}{dy} &= g\rho\frac{dh}{dy}. \end{aligned}\right\} \quad \ldots \ldots \quad (3)$$

Hence for the case of gravitational oscillations (1) becomes

$$\left.\begin{aligned} \frac{du}{dt}-2\omega v &= -g\frac{dh}{dx}, \\ \frac{dv}{dt}+2\omega u &= -g\frac{dh}{dy}. \end{aligned}\right\} \quad \ldots \ldots \quad (4)$$

From (1) or (4) we find, by differentiation &c.,

$$\frac{d}{dt}\left(\frac{dv}{dx}-\frac{du}{dy}\right)+2\omega\left(\frac{du}{dx}+\frac{dv}{dy}\right)=0, \quad . \quad . \quad (5)$$

which is the equation of vortex motion in the circumstances.

These equations reduced to polar coordinates, with the following notation,

$$x=r\cos\theta, \quad y=r\sin\theta,$$

$$u=\zeta\cos\theta-\tau\sin\theta, \quad v=\zeta\sin\theta+\tau\cos\theta,$$

become

$$\frac{D\zeta}{r}+\frac{d(D\zeta)}{dr}+\frac{d(D\tau)}{rd\theta}=-\frac{dh}{dt}, \quad . \quad . \quad . \quad . \quad . \quad (2')$$

$$\left.\begin{aligned}\frac{d\zeta}{dt}-2\omega\tau&=-g\frac{dh}{dr},\\ \frac{d\tau}{dt}+2\omega\zeta&=-g\frac{dh}{rd\theta},\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4')$$

$$\frac{d}{dt}\left(\frac{\tau}{r}+\frac{d\tau}{dr}-\frac{d\zeta}{rd\theta}\right)+2\omega\left(\frac{\zeta}{r}+\frac{d\zeta}{dr}+\frac{d\tau}{rd\theta}\right)=0. \quad (5')$$

In these equations D may be any function of the coordinates. Cases of special interest in connexion with Laplace's tidal equations are had by supposing D to be a function of r alone. For the present, however, we shall suppose D to be constant. Then (2) used in (5) or (2′) in (5′) gives, after integration with respect to t,

$$\frac{dv}{dx}-\frac{du}{dy}=2\omega\frac{h}{D}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or, in polar coordinates,

$$\frac{\tau}{r}+\frac{d\tau}{dr}-\frac{d\zeta}{rd\theta}=2\omega\frac{h}{D}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6')$$

These equations (6), (6′) are instructive and convenient, though they contain nothing more than is contained in (2) or (2′), and (4) or (4′).

Separating u and v in (4), or ζ and τ in (4′), we find

$$\left.\begin{aligned}\frac{d^2u}{dt^2}+4\omega^2u&=-g\left(\frac{d}{dt}\frac{dh}{dx}+2\omega\frac{dh}{dy}\right),\\ \text{and} \qquad \frac{d^2v}{dt^2}+4\omega^2v&=\quad g\left(2\omega\frac{dh}{dx}-\frac{d}{dt}\frac{dh}{dy}\right),\end{aligned}\right\} \quad . \quad . \quad . \quad (7)$$

or, in polar coordinates,

$$\left.\begin{aligned}\frac{d^2\zeta}{dt^2}+4\omega^2\zeta&=-g\left(\frac{d}{dt}\frac{dh}{dr}+2\omega\frac{dh}{rd\theta}\right),\\ \frac{d^2\tau}{dt^2}+4\omega^2\tau&=\quad g\left(2\omega\frac{dh}{dr}-\frac{d}{dt}\frac{dh}{rd\theta}\right).\end{aligned}\right\}\quad . \quad . \quad . \quad (7')$$

Using in (7) (7′), in (2) (2′), with D constant, or in (6) (6′), we find

$$g\mathrm{D}\left(\frac{d^2h}{dx^2}+\frac{d^2h}{dy^2}\right)=\frac{d^2h}{dt^2}+4\omega^2h, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and

$$g\mathrm{D}\left(\frac{d^2h}{dr^2}+\frac{1}{r}\frac{dh}{dr}+\frac{d^2h}{rd\theta^2}\right)=\frac{d^2h}{dt^2}+4\omega^2h. \quad . \quad . \quad (8')$$

It is to be remarked that (8) and (8′) are satisfied with u or v substituted for h.

I. Solutions for Rectangular Coordinates.

The general type solution of (8) is $h=\epsilon^{\alpha x}\epsilon^{\beta y}\epsilon^{\gamma t}$, where α, β, γ are connected by the equation

$$\alpha^2+\beta^2=\frac{\gamma^2+4\omega^2}{g\mathrm{D}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

For waves or oscillations we must have $\gamma=\sigma\sqrt{-1}$, where σ is real.

I *a*. *Nodal Tesseral Oscillations.*

For nodal oscillations of the tesseral type we must have $\theta=m\sqrt{-1}$, $\beta=n\sqrt{-1}$, where m and n are real; and by putting together properly the imaginary constituents we find

$$h=\mathrm{C}\,{\sin \atop \cos}\,\sigma t\,{\sin \atop \cos}\,mx\,{\sin \atop \cos}\,ny, \quad . \quad . \quad . \quad . \quad (10)$$

where m, n, σ are connected by the equation

$$m^2+n^2=\frac{\sigma^2-4\omega^2}{g\mathrm{D}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Finding the corresponding values of u and v, we see what the boundary-conditions must be to allow these tesseral oscillations to exist in a sea of any shape. No bounding-line can be drawn at every part of which the horizontal component velocity perpendicular to it is zero. Therefore to produce or permit oscillations of the simple harmonic type in respect to form, water must be forced in and drawn out alternately all round the boundary, or those parts of it (if not all) for which the horizontal component perpendicular to it is not

zero. Hence the oscillations of water in a rotating rectangular trough are not of the simple harmonic type in respect to form, and the problem of finding them remains unsolved.

If $\omega=0$, we fall on the well-known solution for waves in a non-rotating trough, which are of the simple harmonic type.

I *b*. *Waves or Oscillations in an endless Canal with straight parallel sides.*

For waves in a canal parallel to x, the solution is

$$h=\mathrm{H}\epsilon^{-ly}\cos(mx-\sigma t); \quad . \quad . \quad . \quad . \quad (12)$$

where l, m, σ satisfy the equation

$$m^2-l^2=\frac{\sigma^2-4\omega^2}{g\mathrm{D}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

in virtue of (9) or (11).

Using these in (7), we find that v vanishes throughout if we make

$$l=\frac{2\omega m}{\sigma}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and with this value for l in (12) we find, by (7),

$$u=\mathrm{H}\frac{gm}{\sigma}\epsilon^{-ly}\cos(mx-\sigma t); \quad . \quad . \quad . \quad (15)$$

and using (14) and (13) we find

$$m^2=\frac{\sigma^2}{g\mathrm{D}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

from which we infer that the velocity of propagation of waves is the same for the same period as in a fixed canal. Thus the influence of rotation is confined to the effect of the factor $\epsilon^{-2\omega m/\sigma . y}$. Many interesting results follow from the interpretation of this factor with different particular suppositions as to the relation between the period of the oscillation $\left(\frac{2\pi}{\sigma}\right)$, the period of the rotation $\left(\frac{2\pi}{\omega}\right)$, and the time required to travel at the velocity $\frac{\sigma}{m}$ across the canal. The more approximately nodal character of the tides on the north coast of the English Channel than on the south or French coast, and of the tides on the west or Irish side of the Irish Channel than on the east or English side, is probably to be accounted for on the principle represented by this factor, taken into account along with frictional resistance, in virtue of which the tides of the English Channel may be roughly represented by more powerful waves travelling from west to east, combined with less powerful waves

travelling from east to west, and those of the southern part of the Irish Channel by more powerful waves travelling from south to north combined with less powerful waves travelling from north to south. The problem of standing oscillations in an endless rotating canal is solved by the following equations:—

$$\left.\begin{aligned} h&=\mathrm{H}\left\{\epsilon^{-ly}\cos(mx-\sigma t)-\epsilon^{ly}(\cos mx+\sigma t)\right\};\\ u&=\mathrm{H}\frac{gm}{\sigma}\left\{\epsilon^{-ly}\cos(mx-\sigma e)+\epsilon^{ly}\cos(mx-\sigma t)\right\};\\ v&=0.\end{aligned}\right\} \quad . \ (17)$$

If we give ends to the canal, we fall upon the unsolved problem referred to above of tesseral oscillations. If instead of being rigorously straight we suppose the canal to be circular and endless, provided the breadth of the canal be small in comparison with the radius of the circle, the solution (17) still holds. In this case, if c denote the circumference of the canal, we must have $m=\frac{2i\pi}{c}$, where i is an integer.

II. Oscillations and Waves in Circular Basin (Polar Coordinates).

Let

$$h=\mathrm{P}\cos(i\theta-\sigma t) \quad . \ . \ . \ . \ . \ . \ (18)$$

be the solution for height, where P is a function of r. By (8′) P must satisfy the equation

$$\frac{d^2\mathrm{P}}{dr^2}+\frac{1}{r}\frac{d\mathrm{P}}{dr}-\frac{i^2\mathrm{P}}{r^2}+\frac{\sigma^2-4\omega^2}{g\mathrm{D}}\mathrm{P}=0; \quad . \ . \ . \ (19)$$

and by (7′) we find

$$\left.\begin{aligned} \zeta&=\frac{g}{\sigma^2-4\omega^2}\sin(i\theta-\sigma t)\left(\sigma\frac{d\mathrm{P}}{dr}-2\omega i\frac{\mathrm{P}}{r}\right),\\ \tau&=\frac{-g}{\sigma^2-4\omega^2}\cos(i\theta-\sigma t)\left(2\omega\frac{d\mathrm{P}}{dr}-\sigma i\frac{\mathrm{P}}{r}\right).\end{aligned}\right\} \quad . \ (20)$$

This is the solution for water in a circular basin, with or without a central circular island. Let a be the radius of the basin; and if there be a central island let a' be its radius. The boundary conditions to be fulfilled are $\zeta=0$ when $r=a$ and when $r=a'$. The ratio of one to the other of the two constants of integration of (19), and the speed σ of the oscillation, are the two unknown quantities to be found by these two equations. The ratio of the constants is immediately eliminated; and the result is a transcendental equation for σ. There is no difficulty, only a little labour, in thus finding as many as

we please of the fundamental modes, and working out the whole motion of the system for each. The roots of this equation, which are found to be all real by the Fourier-Sturm-Liouville theory, are the speeds* of the successive fundamental modes, corresponding to the different circular nodal subdivisions of the i diametral divisions implied by the assumed value of i. Thus, by giving to i the successive values 0, 1, 2, 3, &c., and solving the transcendental equation so found for each, we find all the fundamental modes of vibration of the mass of matter in the supposed circumstances.

If there is no central island, the solution of (19) which must be taken is that for which P and its differential coefficients are all finite when $r=0$. Hence P is what is called a Bessel's function of the first kind and of order i, and, according to the established notation†, we have

$$\mathrm{P}=\mathrm{J}_i\left(r\sqrt{\frac{\sigma^2-4\omega^2}{g\mathrm{D}}}\right). \quad . \quad . \quad . \quad . \quad (21)$$

The solution found above for an endless circular canal is fallen upon by giving a very great value to i. Thus, if we put $\frac{2\pi r}{i}=\lambda$ so that λ may denote wave-length, we have $\frac{i}{r}=\frac{2\pi}{\lambda}$, which will now be the m of former notation. We must now neglect the term $\frac{1}{r}\frac{dh}{dr}$ in (19); and thus the differential equation becomes

$$\frac{d^2h}{dr^2}+\left(\frac{\sigma^2-4\omega^2}{g\mathrm{D}}-m^2\right)h=0,$$

or

$$\frac{d^2h}{dr^2}-l^2h=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

where l^2 denotes $m^2-\frac{\sigma^2-4\omega^2}{g\mathrm{D}}$. A solution of this equation is $h=c^{-ly}$, where $y=a-r$; and using this in (20) above, we find

* In the last two or three tidal reports of the British Association the word "speed," in reference to a simple harmonic function, has been used to designate the angular velocity of a body moving in a circle in the same period. Thus, if T be the period, $\frac{2\pi}{\mathrm{T}}$ is the speed; *vice versa*, if σ be the speed, $\frac{2\pi}{\sigma}$ is the period.

† Neumann, *Theorie der Bessel'schen Functionen* (Leipzig, 1867), § 5; and Lommel, *Studien über die Bessel'schen Functionen* (Leipzig, 1868), § 29.

$\zeta = \frac{-g}{\sigma^2 - 4\omega^2} C \sin(mx - \sigma t)(\sigma l - 2\omega m)\epsilon^{-ly}$, where $mx = i\theta$.

Hence, to make $\zeta = 0$ at each boundary, we have $\sigma l = 2\omega m$, which makes $\zeta = 0$, not only at the boundaries, but throughout the space for which the approximate equation (22) is sufficiently nearly true. And, putting for l^2 its value above, we have

$$4\omega^2 m^2 = \sigma^2 \left(m^2 - \frac{\sigma^2 - 4\omega^2}{gD} \right),$$

whence

$$m^2 = \frac{\sigma^2}{gD},$$

which agrees with (16) above.

I hope in a future communication to the Royal Society to go in detail into particular cases, and to give details of the solutions at present indicated, some of which present great interest in relation to tidal theory, and also in relation to the abstract theory of vortex motion. The characteristic differences between cases in which σ is greater than 2ω or less than 2ω are remarkably interesting, and of great importance in respect to the theory of diurnal tides in the Mediterranean, or other more or less nearly closed seas in middle latitudes, and of the lunar fortnightly tide of the whole ocean. It is to be remarked that the preceding theory is applicable to waves or vibrations in any narrow lake or portion of the sea covering not more than a few degrees of the earth's surface, if for ω we take the component of the earth's angular velocity round a vertical through the locality—that is to say, $\omega = \gamma \sin l$, where γ denotes the earth's angular velocity, and l the latitude.

XVII. *On the Resolving-power of Telescopes.* By LORD RAYLEIGH, *F.R.S., Professor of Experimental Physics in the University of Cambridge**.

ALTHOUGH I have recently treated of this subject in the Philosophical Magazine†, its importance induces me to return to it in order to explain how easily it may be investigated in the laboratory. There can be no reason why the experiment about to be described should not be included in every course on physical optics.

The only work on this subject with which I am acquainted is that of Foucault‡, who investigated the resolving-power of

* Communicated by the Author.

† Oct., Nov., and Dec. 1879, Jan. 1880.

‡ "Mémoires sur la construction des télescopes," *Annales de l'Observatoire*, t. v.; also Verdet's *Leçons d'optique physique*, t. i. p. 309.

a telescope of 10 centimetres aperture on a distant scale illuminated by direct sunshine. In this form the experiment is troublesome and requires expensive apparatus—difficulties which are entirely obviated by the plan which I have followed of using a much smaller aperture.

The object, on which the resolving-power of the telescope is tested is a grating of fine wires, constructed on the plan employed by Fraunhofer for diffraction-gratings. A stout brass wire or rod is bent into a horseshoe, and its ends are screwed. On these screws fine wire is wound of diameter equal to about half the pitch, and secured with solder. The wires on one side being now cut away, we obtain a grating of considerable accuracy. A wire grating thus formed is preferable to a scale ruled on paper, and placed in front of a lamp presents a very suitable subject for examination. The one that I employed has 50 wires to the inch, and for security is mounted in a frame between two plates of glass. For rough purposes a piece of common gauze with 30 or 40 meshes to the inch may be substituted with good effect.

For the sake of definiteness of wave-length the grating was backed by a soda-flame, though fair results are obtainable with a common paraffine-lamp. The telescope is a small instrument mounted on a stand, and provided with a cap by means of which various diaphragms can be conveniently fitted in front of the object-glass. The apertures in these diaphragms may be either circular or rectangular. In the latter case the length of the slit is placed parallel to the wires of the grating, and we have the advantage of greater illumination than with a circle of equal width. The observation consists in ascertaining the greatest distance at which the wires can be seen resolved. For this purpose the telescope, focused all the while, is gradually drawn back until in the judgment of the observer the periodic structure is no longer seen; and the distance between the grating and the diaphragm is then measured with a steel tape. The distance thus determined is more definite than might be expected, the differences in the case of various observers not usually amounting to more than 2 or 3 per cent.

Two slits were tried, half an inch long, and of widths ·107, ·196 inch respectively. These widths were measured by inserting a graduated wedge. It was found, however, that the graduations could not be trusted; so that the wedge was in fact used merely to convey the length to be measured to a pair of callipers reading to one thousandth of an inch. The distances at which resolution just ceased were estimated respectively as 91·5 and 168·5 inches, corresponding to angular

intervals between consecutive lines equal to $\frac{1}{4575}$ and $\frac{1}{8425}$. According to theory, the minimum angle is approximately equal to that subtended by the wave-length of light, λ, at a distance equal to the width of the slit, a. In the present case $\lambda = 5{\cdot}89 \times 10^{-5}$ centimetres, and $a = {\cdot}107 \times 2{\cdot}54$, or ${\cdot}196 \times 2{\cdot}54$ centimetres, so that

$$\frac{\lambda}{a} = \frac{1}{4615} \text{ or } \frac{1}{8413},$$

agreeing with the angles found by observation more closely than we should have any right to expect.

Besides these slits, four circular apertures were examined. Their diameters were measured under a magnifier on a glass scale divided to tenths of a millimetre, and were found to be ·172, ·315, ·48, ·63 centimetre respectively. In the case of the two smaller holes the illumination given by an ordinary soda-flame was hardly sufficient; but with the assistance of a jet of oxygen the observation could be made.

The following distances are the means of those found by two observers*—51·5, 98, 149, 196 inches, corresponding to angular intervals $\frac{1}{2575}$, $\frac{1}{4900}$, $\frac{1}{7450}$, $\frac{1}{9800}$ respectively. If D represent the diameter of the aperture, the values of $\frac{\lambda}{D}$ are respectively $\frac{1}{2920}$, $\frac{1}{5350}$, $\frac{1}{8150}$, $\frac{1}{10700}$. The ratios of the observed angular intervals to $\frac{\lambda}{D}$ are thus

$$1{\cdot}13, \quad 1{\cdot}09, \quad 1{\cdot}09, \quad 1{\cdot}09.$$

That a circular aperture would be less effective than a slit of the same width might have been expected. Even in the case of a slit it is advantageous to stop some of the central, in order to increase the relative importance of the extreme, rays; and with a circular aperture the extreme rays are much worse represented than with a slit. From the above results it appears that, to have an equal resolving-power, the circular aperture must be about a tenth part wider than the slit.

Merely to show the dependence of resolving-power on aperture it is not necessary to use a telescope at all. It is sufficient to look at wire gauze backed by the sky, or by a flame, through a piece of blackened cardboard pierced by a needle and held close to the eye. By varying the distance the point is easily found at which resolution ceases; and the observation is

* Mr. Glazebrook and myself.

as sharp as with a telescope. The function of the telescope is in fact to allow the use of a wider, and therefore more easily measurable, aperture.

An interesting modification of the experiment is obtained by using lights of various wave-lengths. For this purpose we may have recourse to coloured glasses; but the best results would doubtless require the rays of the spectrum.

Cavendish Laboratory, Cambridge,
July 9, 1880.

XVIII. *On the Figure of the Planet Mars.* *By* Henry Hennessy, *F.R.S.**

In October 1878 I communicated a paper to the Academy of Sciences in Paris, which was subsequently printed in the *Comptes Rendus*†, in which I established my priority to the discovery of formulæ connecting the polar compression of a planet with its mean density and surface-density; and I drew some conclusions from these formulæ as to the configuration of the planet Mars. Very recently an American astronomer, Professor C. A. Young‡, has published a series of observations made by him of the equatorial and polar diameters of Mars, which seem to have been made under remarkably favourable circumstances. The observations were carefully reduced and corrected for different small disturbing influences; and the final value of e, or the polar compression after all reductions are completed, is $e' = \frac{1}{219}$. It can be easily shown that this value is much more accordant with the theory of former fluidity of the planet, than with the theory of superficial abrasion and denudation by the action of a liquid ocean having the same density as water. If Mars was originally in a fluid state from heat, the mass, in accordance with the properties of fluids, would be arranged in spheroidal surfaces of equal density with a law of increasing density in going from the surface to the centre. The ellipticity or compression would depend on this law and on the periodic time of rotation of the planet, as in the case of the earth. In such a liquid spheroid,

$$e' = \frac{5q'}{2} \mathrm{F}(a'),$$

where q' is the ratio of centrifugal force to gravity at the

* Translation of a paper read to the Académie des Sciences, June 14, 1880. Communicated by the Author.

† *Comptes Rendus de l'Académie des Sciences*, October 22, 1878, p. 590; also Phil. Mag. January 1879.

‡ American Journal of Science, March 1880, p. 206.

equator, and $F(a')$ a function of the radius, whose form depends on the law of density in passing from the surface to centre.

If we denote by T' the time of rotation of the planet, by a' its mean radius, and by M' and g' its mass and intensity of gravitating force at its surface, we shall have

$$q'=\frac{4\pi^2}{T'^2}\frac{a'}{g'},$$

$$g'=\frac{M'}{a'^2},$$

and therefore

$$q'=\frac{4\pi^2}{T'^2}\frac{a'^3}{M'}.$$

Similarly for the earth we have

$$q=\frac{4\pi^2 a}{T^2 g}$$

and also

$$g=\frac{M}{a^2};$$

hence

$$g'=g\frac{M}{M'}\left(\frac{a}{a'}\right)^2,$$

and therefore

$$q'=q\left(\frac{T}{T'}\right)^2\frac{a'}{a}\frac{g}{g'}=q\left(\frac{T}{T'}\right)^2\left(\frac{a'}{a}\right)^3\frac{M}{M'}.$$

Astronomers generally admit that $\frac{a'}{a}=\cdot54$ nearly, $T=86164''$, and for T', 24 hours 37 minutes 22·7 seconds, or $T'=886427''$. If we assume for the masses of the Earth and Mars the values determined by M. Leverrier, we shall have

$$M=\frac{1}{324439}\text{ and }M'=\frac{1}{2812526};$$

and make

$$q=\frac{1}{289},$$

we have

$$2\log\left(\frac{T}{T'}\right)=-1+9753660$$
$$0+9379634$$
$$\log\left(\frac{a'}{a}\right)=-1+1971814$$
$$0\cdot1105708$$
$$\log 289=2\cdot4608978$$
$$-0\cdot1105708$$

$$\log\frac{1}{q'}=2\cdot3503870=224\cdot07\;.\;q'=\frac{1}{224\cdot07}.$$

For the Earth,

$$e=\frac{5}{2}q\mathrm{F}(a);$$

and if $\mathrm{F}(a)$ has the same value in Mars, or, in other words, if the law of density in going from its surface to its centre is the same as in the Earth,

$$\frac{e'}{e}=\frac{q'}{q}, \text{ or } e'=\frac{q'}{q}e.$$

But the latest determination for e gives $e=\frac{1}{293\cdot46}$*. Now $\log 293\cdot46=2\cdot4676969$; and as $\log\frac{q}{q'}=0\cdot1105108$, by subtracting this from the foregoing we have

$$\log\frac{1}{e'}=2\cdot3571861=227\cdot61.$$

Hence

$$e'=\frac{1}{227\cdot61}.$$

As the planet Mars presents evidence of the existence of an aqueous fluid on its surface, a theory sometimes invoked for explaining the earth's spheroidal figure might be appealed to in order to account for the figure of Mars. This is the theory of superficial abrasion and denudation, combined with the centrifugal force resulting from rotation around the planet's axis. This theory has been prominently put forward by Sir Charles Lyell in various successive editions of his 'Principles of Geology;' and although it has been shown to be discordant with the results of mathematical investigation, it seems to hold a place in connexion with the view of some geologists in England and Scotland. It was originally propounded in the latter country by Playfair, whose authority as a mathematician gave it considerable currency. (See Illustration of the Huttonian Theory, Playfair's Works, vol. i. p. 480.)

On the theory of surface-abrasion or surface-moulding of any planet by the action of water, I have found for the ellipticity of the liquid coating,

$$e=\frac{5q\mathrm{D}+6(\mathrm{D}_{,}-1)\mathrm{E}}{2(5\mathrm{D}-3)},$$

where E is the ellipticity of the solid surface, D the mean density, and $\mathrm{D}_{,}$ the surface-density of its solid materials. The

* See Colonel Clarke's paper, Phil. Mag. August 1878, and the recently published 'Geodesy' by the same author.

greatest value which e can have is when $e=\Sigma$; and in this case

$$e=\frac{5q\text{D}}{2(2\text{D}-3)-6(\text{D}_{\prime}-1)}=\frac{5q}{10-6\frac{\text{D}_{\prime}}{\text{D}}}.$$

In the case of the Earth, the values most commonly admitted for the mean density of the planet and its solid crust give in round numbers $\text{D}=5{\cdot}6$ and $\text{D}_{\prime}=2{\cdot}6$. With these numbers it readily appears that e cannot exceed $\frac{1}{417}$.

The smallest value that could be given in the present state of our knowledge to D would make it nearly equal to twice $\text{D}_{\prime}$, and therefore

$$e=\frac{5}{7}q=\frac{1}{404{\cdot}6}.$$

Thus, as I have already pointed out, the abrasion theory cannot account for the Earth's figure as perfectly as the theory of entire original fluidity. If Mars were a homogeneous solid, the abrasion theory would as well account for the observed ellipticity as for a homogeneous fluid; for in both cases e would then be

$$\frac{5}{4}q' \text{ or } e'=\frac{1}{179{\cdot}24},$$

a value which is sensibly larger than the result of the best observations.

The researches of various astronomers have recently shown that the surface of Mars presents a well-defined distribution of land and water. The land seems to be grouped in islands, and not in great continents. Now, if the figure of the planet deviated from that deduced on the hypothesis of original fluidity, if its oblateness were much less or much greater, such a distribution of land and water could not exist. With a large oblateness the land would be arranged in a great belt about the equator; and with a small oblateness or a spherical figure the land would form two circumpolar continents with an intermediate equatorial ocean. All recent observers concur in an entirely different distribution from either of these; and the results of physical theory, measurement of diameters, and graphical representations of the planet's surface seem to be in harmony, and the difficulty which was formerly supposed to exist in accounting for the figure of Mars may be now considered as completely removed.

XIX. *On a Means to determine the Pressure at the Surface of the Sun and Stars, and some Spectroscopic Remarks.* By EILHARD WIEDEMANN*.

IN a former paper I tried to show that we can calculate, at least approximately, the time elapsing between two encounters of the molecules of a gas by measuring the greatest difference in the length of path at which interference of two rays of light coming from the same source of light is possible. I may be allowed here to quote a passage out of that paper†:—

"Two rays of light can only interfere if they emanate from the same source of light, and if there is no sudden change of phase in the source of light during the time elapsing between the two instants at which the first and second ray leave the source. That difference in phase at which interference is still possible is therefore a measure of the time during which no sudden change in phase has taken place at the source of light. A luminous body sends out light coming from a great number of atoms or molecules. Each molecule will only vibrate regularly as long as it does not come within the sphere of action of another molecule; that is, it will only vibrate regularly in the time elapsing between two encounters. For the different molecules, the time elapsing between two encounters may, according to the molecular theory of gases, be either zero or infinitely large. But during a very short fixed time only a small number of molecules will have suffered encounters; only that small number will produce an even illumination of the field of view: the greater number will have vibrated regularly during the whole time; and for all of these interference is possible. The interference-bands in that case are sharp. If, however, the time which we consider to elapse between the emanation of the two interfering rays increases, a greater number of molecules will have suffered encounters, and the bands will therefore be less distinct. It follows that the higher the order of interference-bands, the more diffused and indistinct the bands will be. If the difference of phase corresponds to a difference in time greater than that necessary for the completion of the mean free path, the bands will rapidly disappear, as in that case the greater number of molecules have suffered encounters during the time considered."

Fizeau and Foucault, and, more recently, J. J. Müller and Mascart, have determined the greatest difference of path at

* Communicated by the Physical Society, having been read at the Meeting on June 12th.

† Wied. *Ann.* v. p. 503 (1878); Phil. Mag. [5] vii. pp. 79, 80.

which interference is possible. According to the last-mentioned observer, a difference in path of 50,000, and even 100,000 wave-lengths still produces appreciable interference-bands in sodium light. The time corresponding to this difference of phase is about $0{\cdot}5 \times 10^{-10}$ second. Calculating approximately the mean free path (as we only want to compare the orders of magnitude) for hydrogen at 0° and atmospheric pressure, we find for the time necessary to traverse this free path $1{\cdot}14 \times 10^{-10}$ second. The two numbers are sufficiently close to justify the assumption that the sodium atoms may vibrate during 50,000 oscillations without sudden change of phase. We see at the same time that the disappearance of Newton's bands need not be due to the widening of the lines *producing* them.

As the mean time elapsing between two encounters depends chiefly on the pressure, and far less on the temperature, we have a means of determining approximately the pressure of a gas by an examination of the light which it sends out.

The determination of the quantities relating to large difference of paths in the light sent out by the sun, its protuberances, and stars may give us important information on their physical constitution; and I should like to draw the attention of spectroscopists to this point, now that we may soon expect a renewal of the sunspot maximum.

In order to make the measurements, we need only decompose the light we want to examine by means of a spectroscope, separate a ray, which must be as homogeneous as possible, and count the number of Newton's rings visible between two adjustable pieces of glass. We might also determine the thickness of a plate of Iceland spar which still shows interference-bands if it is placed between two Nicol's prisms. The plate must be cut parallel to the axis; and particular attention must be paid to its homogeneousness.

The spectroscopic evidence hitherto has only related to the presence of a substance in the sun; and we only derive from it very general notions as to the physical state. The radiation of heat is, as Janssen has recently again had occasion to observe, a very complicated phenomenon. We must take account of all the different layers on the sun's surface; and the same difficulty besets the interpretation of the Fraunhofer lines. Their thickness and darkness is a function of at least three variables—temperature, pressure, and thickness of absorbing layer; only after having successfully investigated the separate effect of these three variables, will a more perfect interpretation of the phenomena be possible.

If electric phenomena are going on at the solar surface, the difficulty of the ordinary methods of investigation will be still further increased; for, as I have shown,

(1) Very often, if a spark traverses a mixture of gases, one gas only becomes luminous. This result has recently been confirmed by the photographs of H. W. Vogel.

(2) A gas may, by means of an electric discharge, be made luminous below 100° C.

I have concluded from my investigations that the electric discharge increases the oscillatory energy of a gas independently of its translatory motion; and I compared these phenomena to fluorescence.

Hasselberg has confirmed my results, and has drawn some conclusions from them concerning the aurora borealis, comets, &c. I did not in my first communication refer to these matters, as I hoped first to be able to make some experimental investigations in order to fix the relations existing between pressure, luminosity, and quantity of electricity; but I had thought of these evident applications.

The curious forms of prominences which rise and float freely over the solar surface, and which Lohse has tried to refer to chemical processes, may also be explained by electrical causes.

The two results which I have mentioned must render us very cautious before we apply results arrived at by means of the electric arc or vacuum-tubes to solar phenomena. I believe that we cannot at all employ them for the determination of temperature and pressure.

It is by no means necessary that we should have the same relative intensity of the lines in the spectra produced by the two different causes. The effect of the electric discharge is first of all to displace the æther spheres surrounding the molecules; and the vibrations which are caused are in the beginning independent of the translatory motion, which latter chiefly determines the temperature. By means of the encounters of the vibrating molecules the rotatory motion is changed into translatory motion; and then only is the temperature raised so high that the gas may become luminous owing to its heat. This may take place in the narrow parts of a Geissler tube.

If, on the other hand, we produce spectra by means of heating only without calling electricity to help, we first of all increase the translatory motion, which must be increased considerably; for the gas becomes luminous, and then the changes in the forces binding the atoms together to a molecule must affect the spectrum.

At any rate we must carefully investigate the effects of the electric discharge on the nature of spectra before we can draw any conclusions from spectra produced by electricity on questions relating to temperature and pressure. I intend to discuss this point in another paper.

XX. *On a new Instrument for the Detection and Measurement of Inflammable Gas in Mines.* *By* E. H. Liveing, *Assoc. R.S.M.**

[Plate II.]

THE instruments hitherto contrived for the detection of gas in mines may be divided under two heads:—1st, those which depend for their action on the *Physical* properties of the gaseous mixture; 2nd, those dependent on its *Chemical* or combustible properties.

Under the former head we have the instruments of Mr. Ansell and Prof. Forbes—the one depending on the diffusion of gases, and the other on the velocity of sound; while under the latter head we have the ordinary flame test, the instrument of M. Coquillion, and, lastly, my own instrument. Strong arguments can be urged to show the superiority of the latter class of tests over those of the former type; for it is the heating-value of the gaseous mixture, or its approach towards the explosive proportion, that we really want to know.

It is quite possible to alter the physical properties of a gaseous mixture to a very large extent while its combustible properties remain scarcely changed. Thus the presence of 6 per cent. of marsh-gas in atmospheric air is sufficient to render it explosive in an upward direction, and there may be present in the mixture amounts of CO_2 from 1 up to 15 per cent. without preventing the mixture being explosive.

Atmospheric air containing 8 per cent. CH_4+8 per cent. CO_2 is highly explosive; and yet both the instruments above mentioned as dependent on the physical properties would indicate such a mixture as perfectly harmless, indeed would fail to detect the presence of any combustible gas whatever.

There are other drawbacks to these instruments, but I will not occupy your time by detailing them. On turning to the other class we have, first, the flame test. This has, of course, been the practical every-day test employed in collieries for a century or more past, and was applied at a considerable risk before the introduction of the safety lamp, and since that time with comparative security.

The tail or cap observable upon a flame when brought into an atmosphere contaminated with combustible gas is nothing more than a region where the weak mixture of gas and air receives sufficient auxilliary heat to sustain its ignition-temperature, or, in other words, to burn. It is then a direct combustion test, and therefore on the right principle. There

* Communicated by the Physical Society, having been read June 26th 1880.

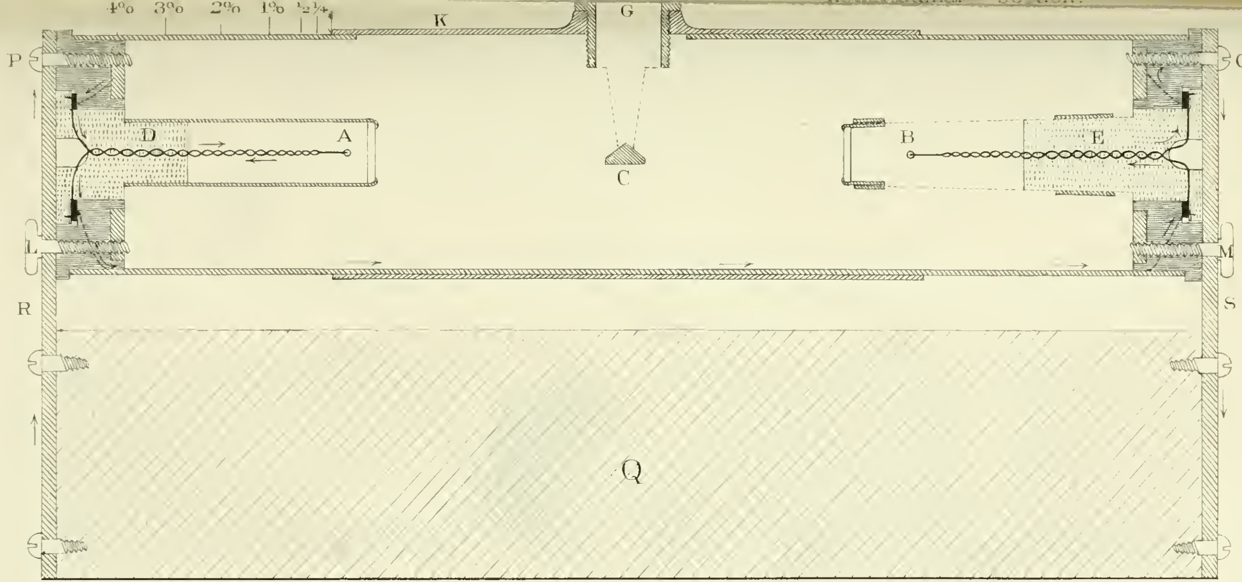
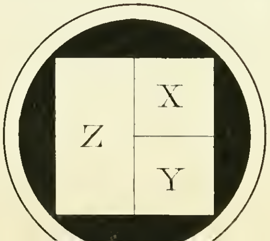


Fig. 2.



are, however, some drawbacks to it: there is a general feeling that it is not sufficiently sensitive; and many hold, further, that there is a wide difference in the percentage of gas that may be present before the cap makes its appearance. Whether this is correct or not, I am not prepared to give a decided opinion; but, from experiments made, I consider 2 per cent. of marsh-gas about the limit detectable with the ordinary Davy flame. A small and clean flame is an essential requisite in applying this test; any particles of ignited matter on the wick will readily produce a spurious cap when no gas exists.

Of the modes of rendering the cap more visible none is more efficient than turning the flame low, or hiding the luminous portion with some opaque object. Attempts have been made to increase the sensitiveness of this test by viewing the flame through blue glass, with a hope that, the yellow colour of the flame being subdued, the bluish cap would be the more apparent; but this has failed practically to increase the sensitiveness of the test, and, I believe, for this reason—that as the percentage of gas is diminished the cap decreases more in size than in intensity; for it is only those molecules that come into very close proximity to the flame that obtain sufficient auxiliary heat to enable them to reach the ignition-temperature.

Of the instrument of M. Coquillion I have nothing to say; for though it may be a very convenient and neat apparatus for gas-analysis, the fact that it cannot be employed in the pit, and that samples of air have to be brought to it puts it entirely out of the present question.

I now come to my own instrument. The principle on which it is based is as follows:—A mixture of marsh-gas and air in which the marsh-gas forms less than 5 per cent. of the mixture is not explosive or capable of continuing its own combustion (at ordinary temperatures and pressures), simply because the heating-value of the CH_4 is insufficient to raise that large excess of atmospheric air to the necessary ignition-temperature. If however, such a mixture is exposed to some sufficiently heated object, especially if that object is platinum, it will burn in its immediate contact and neighbourhood, and in so doing add materially to the temperature of the object, and the more so the larger the percentage of gas present. To apply this principle to the detection and measurement of gas in the air of mines, I have contrived the following arrangement (shown in Plate II.):—A and B are two spirals of very fine platinum wire, through which a current from a small magneto-electrical machine is made to circulate. Both wires being in the same circuit,

and offering equal resistances and cooling-surfaces, become equally heated on turning the machine; one of these spirals is enclosed in a small tube having a glass end and containing pure air; the other is exposed within a small cylinder of wire gauze (also with a glass end) to whatever gaseous mixture is drawn within the instrument for examination.

So long as the air examined is free from combustible gas or matter, the two spirals glow with an equal brilliancy; but if the air contain any amount above $\frac{1}{400}$ of its volume of CH_4 the exposed spiral increases in brilliancy, and the more so the nearer the percentage approaches the explosive proportion; and this difference of brilliancy becomes a means of determining the percentage of gas present.

To measure the difference of brilliancy there is a simple form of photometer, consisting of a small wedge-shaped screen C, the two surfaces of which are covered with paper and illuminated, the one by the covered spiral, the other by the exposed one. These two surfaces are viewed through the small side tube G, which, together with the screen and outer tube K, can be moved towards the covered spiral until equal illumination of the two surfaces of the screen is attained in the particular gaseous mixture under examination. An empirical scaleis then read off, which gives at once the percentage of gas present.

The following table shows the difference of brilliancy due to stated percentages of CH_4; but the amount much depends on the size of the wires, proportion of the spiral, and other conditions, and can only be determined by actual trial in known mixtures. It is a point worthy of note that the rate at which the machine is turned does not seem to have any sensible effect on the ratio of brilliancy of the spirals so long as the exposed one is heated to the igniting-point of CH_4.

Table of observed Illuminating-power of the Spirals in different percentages of gas.

Per cent. CH_4.	Relative illuminating-power of spirals.	
	Covered spiral.	Exposed spiral.
Pure air.	1	1
¼ per cent. CH_4.	1	1·24
½ " "	1	1·65
1 " "	1	2·78
2 " "	1	5·1
3 " "	1	22
4 " "	1	64

When moderately large quantities of gas are present, such as two or three per cent., the difference in colour of the light emitted by the two wires becomes so striking as to cause some difficulty in judging of the exact position of equal illumination of the screen, a difficulty of course felt more or less with all photometers when examining lights of different tint. This difficulty is avoided (so far as the object of this instrument is concerned) by covering one half (Y, fig. 2) of that side of the screen that is illuminated by the exposed spiral with yellowish red paper, the other half, X, remaining white; and for quantities below 2 per cent. the white surface X is compared with the white surface Z; but above that amount the yellow surface Y is employed, its tint neutralizing the very white light of the exposed wire and rendering the comparison easy.

I must now say a few words relative to the practical use of the instrument.

In the modern systems of colliery-ventilation the main current of air that descends the downcast shaft is soon subdivided into several separate currents (or "splits," as they are called). These, passing along the principal travelling-ways, are conveyed to the different districts where the men are working, and, after ventilating a certain number of working-places and becoming more or less contaminated with the combustible gas that is there evolved from the freshly exposed surface of the coal, are carried by air-ways known as "returns" to the upcast shaft, often skirting in this part of their course large areas of broken ground (or goaf) where the coal has been entirely removed.

Now it is for the examination of the air passing in these different returns that I consider this instrument specially adapted, as it at once enables the manager of a colliery to determine the quantity of gas evolved in the different working-districts, and thus regulate the proportion of his various currents of air so as to make the best use of the given ventilating-power that he has at his disposal, and not have some of his air-ways considerably contaminated with gas whilst others are practically free (a condition I have frequently found to be present). Again, if a regular account of the observed percentages of gas be kept, the quantity of air passing and the barometric pressure being also noted, it will soon become evident within what limits it usually varies; and should at any time an abnormal increase be observable, it will be desirable to follow up the return until the source is ascertained and precautions, if necessary, taken.

The instrument thus employed, and the results carefully

considered, cannot fail to be of assistance in laying out and regulating the ventilation of extensive collieries. There is, however, another and perhaps more urgent requirement which I hope a slight modification of this instrument may also be able to meet.

There exist many gassy collieries where safety lamps are alone employed, and yet where the hardness of the coal necessitates the use of powder in order that it may be worked at a profit. Now in such workings, before each shot is fired, it is necessary for the sake of safety (and enforced by the Coal-Mines Regulation Act) that the working-place and its neighbourhood should be carefully tested for gas. This is of course done at present by aid of the lamp, and, it is to be feared, too often without that degree of care and deliberation by which alone the test can be considered efficacious. Be this as it may, all are agreed that some more striking and definite test would be of great value for this purpose, if it could be practically introduced.

For this object I propose a slight modification of the instrument described, in which the screen is fixed at some definite percentage, such as 2 per cent., the one side being marked G, the other A; the directions for use being that so long as the side A of the screen is the brightest the shot may be fired, but if G become equal to or brighter than A the shot should not be fired.

For this purpose the instrument and machine would be best made in one piece, and would have to be constructed as *strongly*, *simply*, and *cheaply* as possible, with an easy means of replacing the platinum spirals, should they be melted through carelessness.

I may add that the chief difficulty about the introduction of the instrument is to find a sufficiently compact and portable source of the necessary electrical current that can be made at a moderate price.

There is yet one other possible application of the instrument —namely, for the examination of the heating-value of the waste gases from blast-furnaces, where it is of importance to regulate the conditions of burning so as to reduce the carbonic oxide in the waste gases to a minimum. Of course the sample of waste gas would have to be mixed with some definite quantity of air, to supply oxygen, before examination.

Explanation of Plate II.

R and S are two strong brass plates that form the terminals of the electrical machine and at the same time act as supports for the instrument.

D and E are wooden plugs with copper wires carrying the platinum

spirals A and B; these are arranged so as to be easily replaced in case of accidental melting or damage. For this purpose the thumb-screws L and M are removed, and the whole instrument turned half round upon the screws O and P; the plugs being in this position released, can be replaced by fresh ones, two small springs making the requisite contacts without trouble. The path of the current is shown by arrows.

There are two small entrance-tubes, through which a breath is taken to fill the instrument with the air to be examined. These are not shown in the figure, not being in the plane of section.

Q is the magneto-electrical machine, the dimensions of which are $8 \times 5 \times 1\frac{3}{4}$ inches.

XXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 67.]

June 23, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Skull of an *Ichthyosaurus* from the Lias of Whitby, apparently indicating a new species (*I. zetlandicus*, Seeley), preserved in the Woodwardian Museum of the University of Cambridge." By Prof. H. G. Seeley, F.R.S., F.G.S.

2. "Note on the Cranial Characters of a large Teleosaur from the Whitby Lias, preserved in the Woodwardian Museum of the University of Cambridge." By Prof. H. G. Seeley, F.R.S., F.G.S.

3. "On the Discovery of the Place where Palæolithic Implements were made at Crayford." By F. C. J. Spurrell, Esq., F.G.S.

The Brickearths of Crayford lie in a channel excavated from the Thanet Sand and subjacent Chalk. The flakes here described were found below the level of the top of the Chalk, on a sort of slope of sand. They form a layer, about 10 feet from N. to S., and 15 feet (perhaps more), from E. to W., which is at one end 36 feet, at another about 42 feet, below the present surface. The flakes lay touching each other, the larger sometimes being several inches thick; they are new and clean, though sometimes studded with calcareous concretions. Some were broken across, evidently before being covered. The author had been enabled to piece many together, and show that the manufacture of haches was the purpose for which they were fractured. Also he had found two pieces of a hache. Fragments of bone were found associated with the flints; among them was part of the lower jaw of *Rhinoceros tichorhinus*. The author regards these Brickearths as slightly newer than the Dartford gravel, which here caps the Thanet Sand, and in which flint implements have also been found.

4. "The Geology of Central Wales." By Walter Keeping, Esq., M.A., F.G.S. With an Appendix by C. Lapworth, Esq., F.G.S., on a new species of *Cladophora*.

The district described by the author is much contorted and dis-

turbed, and offers great difficulties. The following classification of its deposits is proposed, in descending order:—(3) The Plynlimmon Grits; (2) The Metalliferous Slates; (1) The Aberystwith Grits. (1) consists of dark grey grits and imperfectly cleaved slates; they are not very fossiliferous, Graptolites being most abundant. (2) A more argillaceous series of pale blue and grey colour, much folded. This series, near the Devil's Bridge, appears of extraordinary thickness; but the author believes that this is due to a great inversion or, rather, to a series of inversion-folds. Above this is (3) the Plynlimmon group. The area occupied by, and general characteristics of, these groups were described in detail. Fossil evidence enables the author to correlate beds and constitute an order of succession in a considerable number of the sections. All three divisions, however, may be regarded as composing one great group, forming a great primary synclinal, with subordinate anticlinal folds along N. and S. lines. The relation of these beds to the Denbighshire Grits and Tarannon Shales has been investigated in neighbouring districts. The author regards the Plynlimmon Grits as representing a special gritty development in the Tarannon Shales, and so above the Llandovery Grits. The Metalliferous Slates and the Aberystwith Grits, an arenaceous development of their lower parts, represent the Llandovery group of the Survey, probably the Upper and a part of the Lower Llandovery. There does not appear to be any evidence of a break in this district between the Upper and Lower Silurian. This is confirmed by palæontological evidence; and in the study of the Graptolites the author has been assisted by Mr. Lapworth. These show that the Mid-Wales beds are on the horizon of the Upper Birkhill group of Scotland, and of the Coniston Mud-stones of the Lake-district. A table of fossils was appended to the paper, with a description of some new forms. The Appendix, by C. Lapworth, Esq., described a new species of *Cladophora*.

5. "On new Erian (Devonian) Plants." By J. W. Dawson, LL.D., F.R.S., F.G.S.

6. "On the Terminations of some Ammonites from the Inferior Oolite of Dorset and Somerset." By James Buckman, Esq., F.G.S., F.L.S.

7. "Faröe Islands. Notes upon the Coal found at Süderöe." By Arthur H. Stokes, Esq., F.G.S.

The coal in this district is associated with shales; and these are interbedded with sheets of basalt and dolerite. It is worked after a primitive fashion by the natives. Some of the seams are more than half a yard thick. There are two varieties of the coal or, rather, lignite, containing respectively 51·7 and 68·2 per cent. of carbon. The author gave details of sections and other matters connected with the coal-bearing area, and various notes upon the geology of the district.

8. "On some new Cretaceous *Comatulæ*." By P. Herbert Carpenter, Esq., M.A. Communicated by Prof. P. Martin Duncan, M.B., F.R.S., F.G.S.

9. "On the Old Red Sandstone of the North of Ireland." By F. Nolan, Esq., M.R.I.A. Communicated by Prof. Hull, LL.D., F.R.S., F.G.S.

The rock classed on maps of the north of Ireland as Old Red Sandstone is of two kinds—the lower and larger portion chiefly conglomerate of felstone, schist, grit, passing into sandstones, cut off by a fault on N. and N.W. from metamorphic rocks, and resting near Pomeroy, in the N.E., on fossiliferous Lower-Silurian. Associated with these are sheets of lava, probably submarine, from which the above felstone-pebbles have been derived; these are porphyrite. Near Recarson, also, are vesicular melaphyres, whether contemporaneous or intrusive is doubtful. There is also an intrusive granite, which alters the sandstones into quartzites, and is prior to the upper series, now generally held to be basement conglomerate of the Carboniferous. This, formerly coloured as Old Red Sandstone, is unconformable with the other, which it much resembles. The lower conglomerates have been considered Lower Old Red Sandstone; the author showed that these bear great resemblance to parts of the Dingle series of the south of Ireland. In the north of Ireland the upper conglomerates are succeeded by sandstones, and these by Carboniferous Limestone. The author regards the upper conglomerates as representing the Upper Old Red Sandstone of Waterford (the Kiltorcan beds of the south not being identifiable in the north), and the overlying sandstones as the equivalents of the Carboniferous shale and Coomhola grit, and, in Scotland, of the Calciferous Sandstone.

10. "A Review of the Family Vincularidæ, recent and fossil, for the purpose of Classification." By G. R. Vine, Esq. Communicated by Prof. P. M. Duncan, M.B., F.R.S., F.G.S.

11. "On the Zones of Marine Fossils in the Calciferous Sandstone Series of Fife." By James W. Kirkby, Esq. Communicated by Prof. T. Rupert Jones, F.R.S., F.G.S.

12. "The Glaciation of the Orkney Islands." By B. N. Peach, Esq., F.G.S., and John Horne, Esq., F.G.S.

In this paper, which forms a sequel to their description of the Glaciation of the Shetland Isles, the authors, after sketching the geological structure of Orkney, proceeded to discuss the glacial phenomena. From an examination of the various striated surfaces, they inferred that the ice which glaciated Orkney must have crossed the islands in a north-westerly direction, from the North Sea to the Atlantic. They showed that the dispersal of the stones in the Boulder-clay completely substantiates this conclusion; for in Westray this deposit contains blocks of red sandstone derived from the island of Eda, while in Shapincha blocks of slaggy diabase, occurring *in situ* on the south-east shore, are found in the Boulder-clay of the north-west of the island. Again, on the mainland, blocks of the coarse siliceous sandstones which cross the island from Inganess to Orplin are met with in the Boulder-clay between Honton Head and the Loch of Slennis.

Moreover, they discovered in the Boulder-clay the following

rocks, which are foreign to the island—chalk, chalk-flints, oolitic limestone, oolitic breccia, dark limestone of Calciferous-sandstone age, quartzites, gneiss, &c., some of which closely resemble the representatives of these formations on the east of Scotland, and have doubtless been derived thence. From this they infer that, while Shetland was glaciated by the Scandinavian *mer de glace*, Orkney was glaciated by the Scotch ice-sheet, the respective ice-sheets having coalesced on the floor of the North Sea and moved in a north-westerly direction towards the Atlantic.

They also found abundant fragments of marine shells in most of the Boulder-clay sections, which are smoothed and striated precisely like the stones in that deposit. They conclude that these organisms lived in the North Sea prior to the great extension of the ice, and that their remains were commingled with the *moraine profonde* as the ice-sheet crept over the ocean-bed. From the marked absence of shell-fragments in the Shetland Boulder-clay, they are inclined to believe that much of the present sea-floor round that group of islands formed dry land during the climax of glacial cold.

XXII. *Intelligence and Miscellaneous Articles.*

AN APPLICATION OF ACCIDENTAL IMAGES. BY J. PLATEAU, MEMBER OF THE ROYAL ACADEMY OF BELGIUM.

WHEN, on a fine night, with the naked eye we gaze at the full moon near the summit of its course, it is impossible for us to picture to ourselves that a distance of more than 80,000 leagues separates us from it; in spite of ourselves we judge it to be at a relatively very short distance. But what is that distance? It seems, at first sight, very difficult to estimate; the thing is nevertheless possible; and this is how it may be done:—

The absolute size which we attribute to an accidental image is, you know, proportional to the distance of our eyes from the surface upon which we project that image. This results from the fact that the image is due to a modification of a determined portion of the retina, so that it subtends a constant visual angle. Besides, the proportionality in question has been verified by Father Scherfer by means of direct experiments*. If, for example, after contemplating a small red disk placed on a sheet of white paper we cast our eyes upon another part of the paper in order to observe there the green accidental image, this will exhibit the same size as the small disk; but if we move the white paper gradually nearer to our eyes, we shall see the green image diminish proportionally in diameter; if, on the contrary, we turn our eyes to a rather distant wall, the image will appear considerably enlarged. More precisely, the absolute magnitude which we attribute to it is proportional to the distance at which we picture to ourselves the surface on which it is projected.

* *Institutionum opticarum partes quatuor* (Vienna, 1775), pars 1, caput II. art. iii. § 99.

This being the case, let us, at the time of full moon, choose a place of observation sufficiently open, but where there is at least one wall, illuminated either by the moon or by street-lamps. If the sky is clear, let us keep our eyes fixed for some time upon one of the spots of the luminary, situated near its centre, then turn quickly towards the wall in order to project upon it the dark accidental image of the lunar disk. If that image appears to us smaller than the moon itself, let us move further from the wall, but nearer to it if, on the contrary, the image appears larger, and recommence the experiment, until we judge the two diameters equal. This equality evidently demands that we refer the accidental image to the same distance as the luminary; therefore in order to get the distance to which we refer the moon, we shall then have only to measure that which separates us from the wall.

Only I must here note some more or less influential causes of error. In the first place, the exact appreciation of the equality of the diameters of the image and the luminary is, one can understand, very difficult; for the two objects cannot be observed simultaneously. In the second place, we may be mistaken in the idea we form of our distance from the wall, especially if there are no trees or houses to serve as intermediaries. In the third place, clouds floating in the vicinity of the moon would doubtless modify the involuntary judgment we give on the magnitude, and consequently on the distance, of the luminary. It is moreover probable that the distance, estimated in the way we have indicated, would vary with different persons.

My second son, whose sagacity in observation I have proved on many occasions, carried out the experiment under the following conditions:—The house in which I reside looks towards the south; it forms part of one of the long sides of a rectangular "square," part of one of the short sides of which is formed by the wall of an enclosure. On the 23rd of April, the eve of the full moon, at ten o'clock at night (*i. e.* one hour before the moon's passing the meridian) the sky was perfectly clear; and when my son placed himself against our house, he saw the luminary shining in all its splendour at a sufficient altitude above the houses on the opposite side of the square. But as the presence of these might have some influence, he held his hand so as to hide them and them only. After looking at the moon for a sufficient time, he looked at the enclosure-wall of which I have spoken, which was lighted by the lamps of the square; and he moved nearer to or further from it, viewing the moon afresh when the dark image vanished, in order to get the proper distance. For the purpose of determining it with the least error possible, he walked towards the wall till the dark image appeared to him decidedly a little smaller than the moon, then retreated until it appeared decidedly a little larger; and he took the middle point between these two extremes for that which most probably fulfilled the desired condition; moreover the houses on the side of the square near which he was operating constituted intermediaries suitable for giving him a sufficiently distinct idea of the distance of the wall on

which the image was projected. Now the distance to this wall, measured from the point determined as I have said, was found to be 51 metres. Therefore, under the conditions of the experiment, my son instinctively placed the moon in the sky at about fifty metres from him.

That distance will doubtless appear very little; but it is given by the experiment. This, however, ought to be repeated—for which purpose it is not indispensable that the moon be full; but it is necessary that the moon pass the meridian at a favourable time, and, further, that the sky be quite calm and clear—circumstances not at our command. Perhaps the hand concealing the houses diminished the apparent distance; an observer residing in the country would without difficulty find more favourable conditions. At all events, if any one repeats the experiment, I would advise him to be careful of his eyes—that is to say, not to look at the moon longer than is necessary to get a quite distinct accidental image, and not to make too many trials; for my son, who, no doubt, was deficient of prudence in that respect, experienced on the morrow a rather intense irritation of one of his eyes.—*Extrait des Bulletins de l'Académie Royale de Belgique*, 2me série, tome xlix. no. 5, May 1880.

ON BOLTZMAN'S METHOD FOR DETERMINING THE VELOCITY OF AN ELECTRIC CURRENT. BY E. H. HALL.

In the June number of Silliman's Journal is mentioned a note relative to the velocity of electricity, published by Prof. Boltzman in the *Kaiserliche Akademie der Wissenschaften in Wien*, Jan. 15th, 1880*. In this note Prof. Boltzman points out a method by which, as he thinks, the absolute velocity of current-electricity may be determined from the results furnished by the study of a phenomenon lately described in Silliman's Journal† under the title, "A New Action of the Magnet on Electric Currents." Quite recently there has appeared in the *Kaiserl. Akad.* an account of experiments and calculations made by Albert v. Ettingshausen, whereby he deduces for the electrical current sent by "one or two Daniell's cells" through his strip of gold the velocity 1·2 millim. per second.

Unless I have misunderstood Prof. Boltzman's note, however, there is a fatal objection to the fundamental assumption which he makes. I will give very briefly his method of reasoning, as I understand it.

We know, as Prof. Boltzman says, that a conductor bearing a current is acted upon by a force tending to move it in a direction at right angles to the direction of the magnetic force acting upon it. We know, moreover, from the new phenomenon, that there is at the same time a difference of potential set up between points on opposite sides of the conductor, and that the electromotive force thus arising is in the same line as the above force acting upon the conductor.

* Phil. Mag. April 1880, p. 307.
† Phil. Mag. March 1880, p. 225.

Consider now any particle of electricity in the conductor. It is acted upon by the newly-discovered transverse force, tends to move accordingly, and tends to draw the conductor with it. Imagine enough particles of electricity crowded into the conductor, and we have the explanation of the familiar action between magnets and conductors bearing currents. Knowing, therefore, the strength of our magnetic field, the strength of the primary current, and the consequent difference of potential on opposite sides of the conductor, we can calculate exactly the amount of electricity contained in unit length of the conductor at any moment while the current is flowing. Knowing, moreover, the amount of electricity passing through the conductor in unit of time, which quantity is of course what we call the strength of the current, it is a perfectly simple matter to determine the velocity of the current.

This question meanwhile presents itself:—If the very slight difference of potential existing between opposite sides of the conductor is sufficient, when acting upon the electricity contained within the conductor, to cause the strong action which every one has observed between magnets and conductors bearing currents, why is there not an enormously greater force always acting upon the conductor in the direction of the primary electromotive force and primary current?

To get a more definite view of the matter, suppose we send through a strip of gold leaf, a centimetre wide and of any length, a current of strength ·05 (cm.-grm.-sec.), and place the strip in a magnetic field of strength 4000. A certain difference of potential would now be observed between points opposite each other on the edges of the strip. This difference of potential, E′, would, in the case imagined, be perhaps $\frac{1}{3000}$ the difference of potential, E, for two points a centimetre apart in the line of the main current. Now the force acting upon a unit length of the conductor to move it across the lines of magnetic force would be $4000 \times \cdot 05 = 200$ dynes.

This force everybody knows to exist. Let us suppose for the moment, with Prof. Boltzman, that it is due to the difference of potential E′ acting upon the electricity in the conductor. But now we have the difference of potential E, 3000 times as great as E′, acting upon the same electricity, but acting in the direction of the current.

To be consistent, therefore, we must look for a force in this direction equal to $3000 \times 200 = 600{,}000$ dynes, a force equal to the weight of about 600 grams, acting upon each unit length of the gold-leaf strip. Thus, in following out Prof. Boltzman's assumption to what seems to me its necessary consequences, we are led to a manifest absurdity.

Another objection to the above assumption, and a serious one apparently, is found in a fact not known to Prof. Boltzman when his note was written.

The transverse electromotive force in iron is opposite in direction to that in gold. According to the theory proposed, therefore, an iron wire bearing a current should move across the lines of mag-

netic force in a direction contrary to that followed by wires of other materials, which it does not do.

In view of these difficulties, it seems hardly possible at present to accept Prof. Boltzman's method of calculating the velocity of electricity.

Any one desiring to see Prof. Boltzman's note wil. find a translation of the same in the Philosophical Magazine of April 1880, p. 307. A rather confusing inaccuracy in translation is, however, to be found about the middle of page 308, in the sentence, "Hence, if the force above denoted by k *itself* acts upon &c. This should read, "Hence, if the force above denoted by k acts upon the movable electricity *itself* in the gold-leaf &c." The position of the pronoun is here a matter of considerable importance, as any one will see who reads Prof. Boltzman's note with care.—Silliman's *American Journal*, July 1880.

ON A NEW FORM OF GALVANOMETER. BY L. GOSTYNSKI.

I have the honour to introduce to the Academy a new galvanometer for thermoelectric currents, which is distinguished particularly, from all those with which we are familiar, by the combination of two astatic systems having the same direction. The chief advantage of this apparatus consists in the proportionality, which I have been able to extend up to nearly 90°; this renders unnecessary the construction of Tables, which, besides, are often inadequate.

Having to make and to verify a great number of determinations respecting the transmission of heat through various thicknesses of water, I sought for means of measurement at the same time simple, convenient, and precise. The apparatus in question combines these conditions, and can be rendered highly sensitive. It has a continuous induction-coil—that is, without a slit for the passage of the astatic system. A U-shaped aluminium wire, suspended by a cocoon-thread, supports two astatic systems, both having the same direction, crossing at an angle of about 45°, and joined one to the other. In a small vertical mirror surmounting the U-wire, and carried along by the double astatic system in its motion of rotation under the action of the current, the divisions of a semicylindrical scale having the cocoon-thread for its axis are reflected, and project themselves upon a small vertical fixed sight placed behind the mirror. The zero of the scale corresponds to that position of the U-wire for which one of the astatic systems is parallel to the turns of the coil, the direction of the current being such that the other system moves towards the starting-point of the first.

For nearly two months I have been verifying the proportionality of various deflections to the left and to the right of zero; and that proportionality has been confirmed by more than fifty series of cross observations, each series comprising at least six partial determinations.

In concluding this compendious sketch, I consider it a duty to express my deep gratitude to MM. P. Desains and J. Jansen, who have promoted and encouraged my researches; and I must also record the obliging cooperation of the house of Carpentier, and in particular of M. Guerout, who has directed, with much care and kindness, the construction of my apparatus.—*Comptes Rendus de l'Académie des Sciences*, June 26, 1880, t. xc. p. 1534.

ON A DIGESTIVE FERMENT CONTAINED IN THE JUICE OF THE FIG-TREE. BY M. BOUCHUT.

The researches which I and M. Ad. Wurtz presented to the Academy, on the digestive action of the juice of *Carica papaya*, and of the digestive ferment (papaïne) which it contains, have induced me to see if that was not a fact connected with a *general carnivorous property of the latex* of many other plants. Special studies, carefully made, in that direction compel me to think so; and now, at least, the thing seems demonstrated by the milky juice of the common fig-tree.

This juice is not very copious, its collection slow and difficult; no one has any great quantity of it. Nevertheless I obtained from Provence a remittance of the latex collected in the month of April —which it is important to remember, as the quality of the juice changes with the more or less advanced state of the vegetation; and in the laboratory of M. Wurtz we made some experiments, which have given the following results:—

Five grams of the milky juice, in part coagulated—forming a serous portion, and a white, glutinous, elastic, and perfumed resinous coagulum—were kept, in a glass with 60 grams of distilled water and 10 grams of moist fibrine, at a constant temperature of 50° C. After some hours the fibrine was attacked, softened; and in the evening it was digested, leaving a little white residue at the bottom of the glass.

I added successively, in the same glass and the same liquid:— first, 10 grams of moist fibrine, which were digested in twelve hours; next, 12 grams, and then 15, and that eight times at intervals of one or two days, always taking care to keep the vessel at the same temperature. These different additions amounted to no less than 90 grams of fibrine for one month of experiment.

Each quantity of fibrine was digested in less than twenty-four hours, and left a homogeneous whitish residue, which was added to that of the preceding digestion. The solution had a pronounced odour of good broth, without the slightest putridity and with a pleasant smell, due to the resinous coagulum of the fig-tree juice left designedly in the glass.

At the end of a month we discontinued the experiment. The fibrine digestions had not fermented; they retained a savoury smell of digested meat, plus the aroma of the fig-tree resin. Other similar experiments have yielded the same results. They prove

that there is in the latex of the fig-tree a powerful digestive ferment; and we hope shortly to be able to state what is the composition of the residue and what the nature of this new vegetable pepsine principle which is capable of thus digesting albuminoid substances.—*Comptes Rendus de l'Académie des Sciences*, 5th July, 1880, t. xci. p. 67.

OBSERVATIONS ON THE VAPOUR-DENSITY OF IODINE. BY M. BERTHELOT.

In the abstract theory of gases it is assumed that the simple gases receive simultaneously one and the same increment of total energy and one and the same increment of *vis viva* of translation when they undergo one and the same change of temperature. This conception translates the experiments of physicists on the specific heat of gases (the law of Dulong and Petit), their expansion by heat (Gay-Lussac's law), and their compressibility (Mariotte's law).

Again, it is concluded from the two latter laws that the density of a gas (that is, the ratio between the weight of a given volume of it and the weight of the same volume of air), taken at the same temperature and the same pressure, is, as a principle, constant. The deviations hitherto observed have been attributed to secondary perturbations.

These three laws have really been demonstrated for three elements only (oxygen, hydrogen, nitrogen); they constitute the only scientific foundation on which the physical determination of molecular weights, and consequently the numeration of the atoms, in present theories rests. If for certain elements these laws were to cease to be true, in that case the physical definition of the molecular weights of those elements and that of the number of their atoms would become pure conventions.

Now I have already pointed out that the experiments of MM. Kundt and Warburg on the velocity of sound in mercurial gas were irreconcilable with the whole three fundamental laws above recapitulated (*Annales de Chimie et de Physique*, 5e série, t. ix. p. 427).

The experiments of M. V. Meyer on the diminution of the gaseous density of iodine and the halogen elements under constant pressure, but at temperatures very remote from one another, are still more opposed to the received laws. These experiments are moreover confirmed and extended by those which M. Troost has just performed on the same body, at a constant temperature but under various low pressures, with the great precision which characterizes him*.

Thus the variation of the *vis viva* of translation of the molecules of gaseous iodine, under the influence of very high temperatures or of very low pressures, far surpasses the same variation, observed

* *Vide infrà*, p. 141.

under the same conditions, in the molecules of air. The laws of Mariotte and Gay-Lussac, established solely upon three simple gases, are therefore inapplicable either to iodine or to the other halogen elements.

This is the place to mention that the law of specific heats is no more applicable than the above-mentioned to this group of elements; for the specific heats of gaseous chlorine and bromine exceed by one fourth those of the other simple gases, and that between the ordinary temperature and 200° C., temperatures at which dissociation cannot be admitted.

It hence follows that the increase of the total energy of the halogen gases with the temperature exceeds that of the three other simple gases hitherto investigated (nitrogen, oxygen, hydrogen), as well as the increase of the *vis viva* of translation: these two orders of effects seem to be correlative.

Moreover, the diminution of density of iodine gas being progressive, the same is the case with the augmentation of its *vis viva* of translation; and, as M. Troost very judiciously remarks, this does not permit us to draw any correct conclusion respecting the variation of the number of the molecules; that sort of reasoning becomes arbitrary the moment the weight of the molecule of iodine, viewed either at a high temperature or at a low pressure, eludes the old definitions.

Only one law remains applicable to the elements, possessing an absolute and universal character; it is the invariableness of the proportions by weight in which the elements combine with one another. That is now the only immovable foundation of chemical science.—*Comptes Rendus de l'Académie des Sciences*, July 12, 1880, t. xci. pp. 77, 78.

ON THE DENSITY OF THE VAPOUR OF IODINE. BY L. TROOST.

The highly important researches published by M. V. Meyer on the variation of the density of iodine-vapour at very high temperatures, and the results obtained by MM. Crafts and Meyer which confirm them, have decided me to take up those densities again, with the apparatus made use of by M. H. Sainte-Claire Deville and myself for the vapour-densities of selenium and tellurium, and in which we determined the temperature with the aid of the air-thermometer*.

I have employed, as in those old experiments, balloons of porcelain, glazed inside and outside, and of a capacity of from 250 to 300 centims. They are tared with the little porcelain stopper, which at the moment of closing will be fused at the oxyhydrogen blowpipe.

* The supposition which has appeared in a recent publication, that we did not employ the air-thermometer for the determination of temperatures above that of the boiling-point of zinc, is erroneous.

The balloon containing the iodine is introduced into a horizontal muffle made of fireclay; this is placed in a stove heated by heavy coal-oil, which comes through a graduated and very delicate cock. For determining the temperature, I have utilized the new air-thermometer described by M. H. Sainte-Claire Deville and me at the meeting of the 29th of March last. The following are the results of the experiments, made at elevated and easily-obtained temperatures:—

	I.	II.	III.
Volume of the balloon	269·4 cub. cent.	255 cub. cent.	252 cub. cent.
Temperature of the balance	16°·5	16°·5	15°·8
Atmospheric pressure	756·14 millims.	755 millims.	757 millims.
Increase of weight	−0·056 gram.	+0·008 gram.	−0·021 gram.
Gas remaining, measured moist...	16·1 cub. cent.	5·1 cub. cent.	8·6 cub. cent.
Temperature	19°	27°	18°·5
Pressure	753·4 millims.	745 millims.	757 millims.
Gas drawn from the reservoir ...	14 cub. cent.	14·2 cub. cent.	13·5 cub. cent.
Temperature	21°·4	27°	16°·2
Pressure	541·4 millims.	544·4 millims.	534 millims.
Volume of the reservoir.........	46·24 cub. cent.	46·24 cub. cent.	46·24 cub. cent.
Gas drawn from the compensator	1·38 ,,	1·59 ,,	1·40 ,,
Temperature	21°·6	27°·7	16°
Pressure	438 millims.	440 millims.	434·3 millim.
Temperature deduced............	1235°·5	1241°	1250°
Density, obtained by means of the coefficient of expansion of the air	5·82	5·71	5·65

The numbers given in this Table for the vapour-density of iodine were calculated by assuming that that vapour possesses a constant coefficient of expansion and equal to that of air. Is it right to make that assumption? I thought it necessary, in order to solve this question, to make other experiments. These are the results which I obtained by taking the densities at the constant temperature of the ebullition of sulphur, but under variable pressures:—

	I.	II.	III.	IV.	V.
Volume of the balloon	334 c.c.	281 c.c.	295 c.c.	320·3 c.c.	310 c.c.
Temperature of the balance	9°·5	19°·8	20°	20°	18°·8
Atmospheric pressure	768·5 mm.	758·82 mm.	755·72 mm.	764·6 mm.	758 mm.
Excess of weight	+10·01 gr.	−0·238 gr.	−0·286 gr.	−0·3035 gr.	−0·325 gr.
Air remaining	4 c.c.	0·7 c.c.	0·6 c.c.	0·63 c.c.	0·6 c.c.
Temperature..............	9°·5	23°	22°·5	22°·8	24°
Pressure......................	768 mm.	676·8 mm.	495 mm.	735·9 mm.	485·5 mm.
Presure at closing the balloon	768 ,,	67·2 ,,	48·6 mm.	48·57 mm.	34·52 mm.
Density obtained on applying Mariotte's law..	8·70	8·20	7·75	7·76	7·35

The numbers given in this second Table were calculated on the hypothesis that iodine vapour follows Mariotte's law exactly.

These results show that the vapour-density of iodine, calculated with $\alpha = 0 \cdot 00367$ and $PV = 1$, diminishes quite as much at a low as at a high temperature.

All the hypotheses which have been framed on the assumption of either a dissociation of, or isomeric change in, iodine henceforth appear to me hardly admissible. In the present state of our knowledge, nothing authorizes us to suppose that a partial vacuum would be adequate to produce a modification of that nature. The only necessary consequences of the experiments made at high temperatures or at low pressures are, that the expansion-coefficient of iodine varies with the temperature, and that its coefficient of compressibility varies with the pressure. Every hypothesis proposed in order to explain these results will have to take into account this double variation.—*Comptes Rendus de l'Académie des Sciences*, July 5, 1880, t. xci. pp. 54–56.

NOTE ON A DEMONSTRATION-DIFFERENTIAL THERMOMETER. BY HENRI DUFOUR.

For the purpose of exhibiting, in lectures on physics, the principal phenomena due to the radiation of heat, the thermoelectric pile and a galvanometer are usually employed. If the latter is a reflecting one (such as that of M. Wiedemann), it is easy to render visible to a numerous auditory some of the most delicate thermal phenomena. The only inconvenience of these two instruments is their high price; perhaps it is on account of this that the study of the radiation of heat is so often neglected in colleges which possess but slender resources. It is in order to render the study of these phenomena possible to every one, that I have constructed the following instrument, which can be easily executed anywhere at a very moderate price.

A tube in the shape of a very widely open V (the two branches making an angle of about 140°) is terminated at one of its ends by a blackened bulb. A horizontal lever of very light wood unites the two branches as the bar of an inverted A ($\forall$) would do; this lever turns on a horizontal axis fixed to the middle of its length; upon the axis is a vertical needle, which moves in front of a graduated dial, likewise vertical.

A short column of mercury is introduced into the tube so that it occupies its lower portion. Equilibrium being established, the indicating needle is at the zero of the graduation. Under these conditions any heating of the bulb produces expansion of the air which it contains, and consequently a displacement of the mercury index, under the influence of which the apparatus inclines more or less; it afterwards returns to zero when the action of the source of heat ceases to operate.

To regulate the horizontality of the beam, a small brass cursor can be placed on the lever at a variable distance from the axis. Lastly, the motion of the apparatus is very regular if the precaution

be taken of introducing into the tube containing the mercury a small quantity of concentrated sulphuric acid to protect the terminal surfaces of the mercury from oxidation.

In order to employ this apparatus for the study of the phenomena of radiant heat, a polished brass cone is placed inside at a short distance from the bulb, the diathermanous substances to be investigated being fixed between the small aperture of the cone and the bulb; the heat of a wax taper, sent back by a small reflector, suffices for most experiments. The absorptive and emissive powers of the various substances are easily studied by preparing a certain number of small disks of copper (such as the circular copper plates of a Volta pile); each disk is coated on one side with lampblack, on the other with the substance whose absorbing-power is to be determined. All the disks being heated together in a metal vessel, we have only to place them successively at the same distance from the bulb, the substance to be studied facing the bulb, in order to determine the emissive powers.

If the same disks be placed all at the same distance from a stove or any source of heat, the different faces being turned towards the source of heat, and be then placed successively each with its black face looking towards the bulb, the inequality of their absorbing-powers is shown by the inequality of the deflections of the instrument.

As will be seen, the instrument is nothing else but a differential bulb-thermometer; but its indications are more easily read at a distance than those of Rumford's or Leslie's apparatus. Of course, as the apparatus varies with the barometric pressure and the temperature of the surrounding medium, the horizontality of the lever must be established, by shifting the cursor, before commencing a series of experiments.

This apparatus may be made of very various dimensions. That which I use has its lever about 14 centims. in length, the mercury index 5 centims. I have constructed another much smaller, of only 3 centims. length, the bulb of which has a diameter of only about 5 millims., and the tube is almost capillary. With the instrument it is very easy to verify the distribution of the heat in the solar spectrum. It can also be constructed with two bulbs, like the ordinary differential thermometer; but it is then less sensitive*.—*Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, tome iv. no. 7, pp. 71–73.

* At the time I constructed the above-described instrument I knew nothing about M. Marey's thermograph (see *Méthode graphique dans les sciences expérimentales*, par M. E.-J. Marey, p. 314). This appears to me more difficult to construct than that which I propose; but it has the advantage of possessing a fixed bulb.

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XXIII. *On the Behaviour of Liquids and Gases near their Critical Temperatures.* *By* J. W. CLARK*.

[Plate III.]

IN 1822 Baron Caignard de la Tour † showed that the effects of heat on a liquid enclosed in a vessel adapted to the purpose, was to convert it into vapour at a volume a little more than twice that which it originally possessed. Within a certain limit he found the temperature at which this change occurred was independent of the ratio existing between the volume of the liquid and that of the tube, but above that limit the conversion was first observed at a higher temperature. Brünner ‡, after an extensive discussion of previous memoirs, states the results of his observations upon the decrease which heat produced in the height to which water, ether, and olive oil rose in capillary tubes. He experimented at temperatures below 100° C. In 1857, from Brünner's expression for this decrease in the case of ether, Wolf § calculated the temperature at which the level of the liquid in the capillary tube should coincide with that outside it; and in the attempt to test his conclusion experimentally he found that it sank below the liquid in the external tube. He stated that the surfaces became convex, and that the depression was the result of true

* Communicated by the Physical Society.

† *Ann. de Chim. et de Phys.* t. xxi. p. 127 and p. 178; *ibid.* t. xxii. p. 411.

‡ Pogg. *Ann. der Phys. u. der Chemie,* Bd. lxx. S. 481.

§ *Ann. de Chem. et de Phys.* t. xlix. p. 272.

capillary action, as in the case of mercury. Drion * then took up the subject, and ultimately came to the conclusion that the depression of the liquid in the capillary tube just before its vaporization was the result of a less rapid expansion produced by a difference of temperature too small to detect with a thermometer. He described the surface of the liquid at the moment of disappearance as perfectly plane, and the apparent convexity as the result of refraction.

Mendelejeff † appears to have misunderstood Wolf; for in 1870 he writes :—" Wolf hat im Jahr 1858 (*Ann. de Chim. et de Phys.*, t. 49, p. 259) gezeigt dass bei der Temperatur bei welcher Aether in zugeschmolzenen Röhren ganz in Dampf verwandelt wird der Meniscus verschwindet und dass Niveau des Aethers in der Capillarröhre und in der weiten Röhre gleich ist. Die Beobachtung wurde von Drion (*ibid.* 1859, t. 56, p. 221) bestätigt und erweitert." The researches of Dr. Andrews ‡ are too well known to need special reference here ; and the discussion of a paper by Dr. Ramsay §, on the " Critical State of Gases," will be best left until the completion of the work in part described in this paper. Messrs. Hannay and Hogarth ‖ have recently shown that under certain conditions solids may be dissolved in gases. Unfortunately, Mr. Hannay's paper, " On the Cohesion Limit," recently communicated to the Royal Society, is not yet printed; so that reference to it must be left until a future occasion.

In December 1878 I read a short preliminary note before the Society, " On the Surface Tension of Liquified Gases," in which the results of some measurements on sulphurous anhydride at low temperatures were given ¶. Last year, at the Society's meeting at the Royal Indian Engineering College in June, a curve showing the height at which sulphurous anhydride stands in a tube at temperatures between $-17°$ C. and the critical temperature was shown, and the depression of the liquid in a capillary tube, with some unsuccessful attempts to determine the cause of it, were described. As this is the last meeting of the session, I beg leave to lay the result of the inquiry before the Society.

* *Ann. de Chim. et de Phys.* 1859, p. 221 ; and *Comptes Rendus*, t. xlviii. 1859.

† Pogg. *Ann.* 1870, Bd. 141, S. 621.

‡ Phil. Trans. 1869, p. 575 ; *ibid.* 1876, p. 421.

§ Proc. Roy. Soc. vol. xxx. p. 323.

‖ *Ibid.* xxix. p. 324 ; *ibid.* xxx. pp. 178 and 188.

¶ I have since found that the value then given is considerably too low, no correction for the diameter of the external tube having been made. To Professor Quincke I am greatly indebted for having directed my attention to the previously reasoned observations of Wolf and Drion here quoted, and for having kindly supplied me with most of the above references.

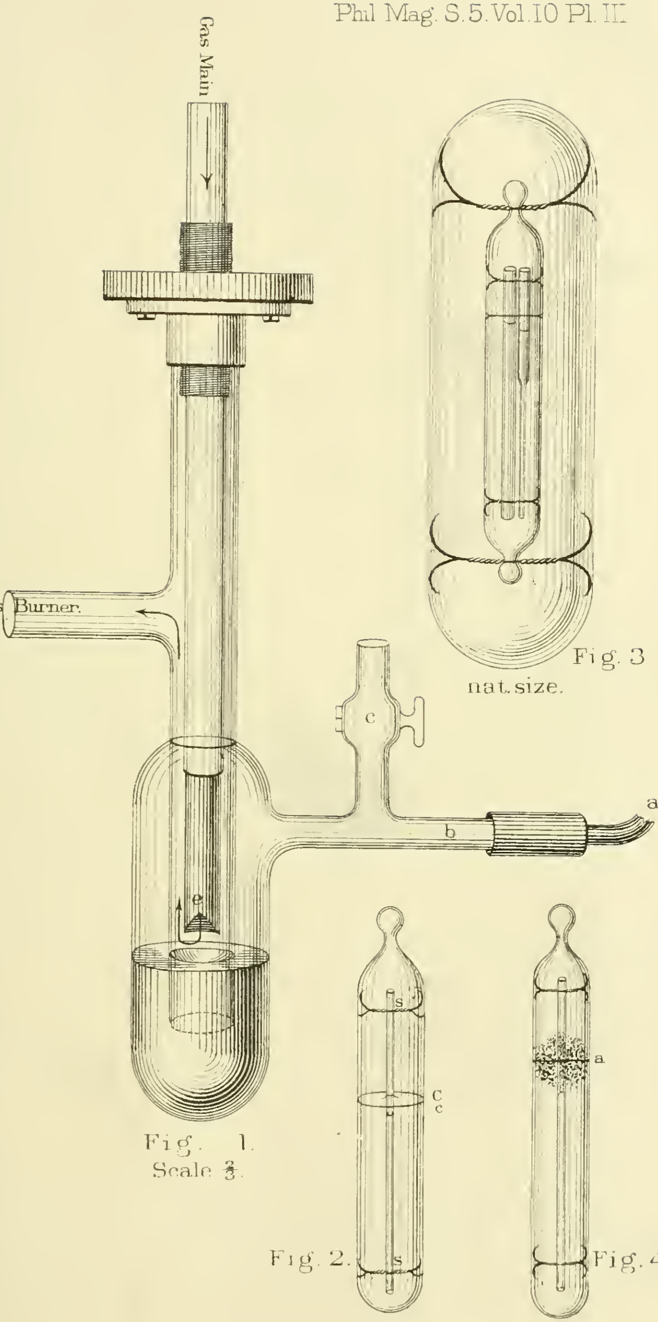


Fig. 1.
Scale $\frac{2}{3}$.

Fig. 3
nat. size.

Fig. 2.
nat. size

Fig. 4.
nat. size.

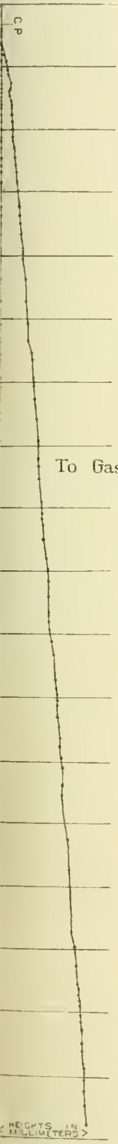


Fig. 5.

Two methods of heating will be noticed in this paper. The first consists merely in heating a large test-tube of oil over a rose gas-burner, the experimental tube being suspended vertically in its axis a little above the bottom by a fine platinum wire. Numerous experiments have been tried with U-tubes of which the branches were of *equal diameter and glass-thickness*; and the results show that the heating, although rapid, is nearly, if not quite, uniform. It is worth notice, however, that at temperatures very near the critical, the density of liquid and vapour are so nearly equal that when the heating is made unequal, the entire disappearance of the liquid from one branch of the tube does not affect its level in the other. The second method of heating is more complex, but has answered the purpose sufficiently well to deserve brief description. In the middle of a four-litre beaker of oil* is fixed a round-bottomed thin flask of 1·5 litre capacity. The neck, which is about 3 centims. in diameter, is thickly wrapped round with cotton wool and calico, and is long enough to rise 7 centims. above the perforated cover which closes the beaker. The beaker rests on wire gauze carried by a large iron ring, and is surrounded by a tin cylinder 8 centims. wider than the beaker, furnished with two small glass windows on opposite sides. The neck of the flask passes through the cover of this outer case, and is closed air-tight by an indiarubber cork, through which the bulb of a long-stemmed thermometer is inserted. Just below the cork is fused into the neck a narrow glass tube connected with a slightly modified Bunsen regulator (fig. 1, Plate III.) by a short piece of composition pipe (a) 2 millims. in diameter †. The other end communicates through a short glass tube (b) with the air-mercury bulb of the usual regulator. A small glass stop-cock (c) serves to place the flask and regulator-bulb in communication with the air. A large rose burner protected from air-currents heats the beaker of oil.

When the apparatus is in use the experimental tube is suspended in the middle of the flask by a thin wire, the glass stop-cock (c) of the regulator opened, and a small gas-flame lit beneath the beaker. When ether is used in the experiment, in from eight to ten hours the temperature rises to within

* The best salad oil heated out of contact with such metals as copper and iron will bear repeated heating to 200° C. It ultimately becomes thicker and denser, and exhibits a green fluorescence. By long exposure to the action of air and light, particularly in thin sheets, it regains its transparency and again becomes fit for use. It should be noticed that some sorts of glass are violently attacked by hot oil.

† These regulators may be obtained from Mr. Bel, 34 Maiden Lane, Strand.

three or four degrees of the critical; the glass stop-cock (c) is then closed, and the pressure of the expanding air in the flask begins to raise the mercury until it meets the platinum tube (e) of the regulator, which is previously screwed down till within about 3 millims. of the surface of the mercury. Some two or three hours later the critical temperature is reached. The light from a paraffin lamp passes through a cylindrical lens, and enters through one of the glass windows in the case enclosing the apparatus, thus rendering the experimental tube visible through the other window. The observations are made with a cathetometer-telescope.

Early in this inquiry it was found that so many circumstances influenced the depth to which the liquid was depressed in the capillary tube, that two different tubes could be satisfactorily compared only when enclosed in the same external tube; and if the effects of slow and rapid heating are to be compared, the tubes should be of the same thickness of glass. Small springs of thin platinum (S, fig. 2) fix the capillary tubes vertically in the axis of the external tubes, which latter have been employed of various diameters between 2 and 20 millims. When the slow heating-apparatus is used, the experimental tube (enclosing the capillary tube) is fixed in the axis of a tube of considerably greater diameter, which is then exhausted of air and hermetically sealed (see fig. 3). Ether distilled from calcic chloride has been the liquid most frequently employed; but, so far as I am aware, all the results about to be described are also obtained with alcohol, sulphurous anhydride, and carbonic disulphide. Water attacks glass so rapidly that it is difficult to ascertain what is taking place in the tube; but it probably forms no exception to the liquids mentioned.

After the introduction of the capillary, the external tube is filled with liquid, and the air removed as completely as possible by repeated boiling, and the volume of the liquid is reduced to a convenient extent. When such a tube contains ether and is heated, the liquid sinks in the capillary tube and rises in the outer, the expansion becoming more and more rapid as the critical temperature is approached. About 2° C. below this temperature the meniscus in the capillary tube stands at the same level with that in the external tube. Both surfaces are then distinctly concave. In the case of alcohol the temperature at which this is observed is *probably* a little more, and for sulphurous anhydride a little less, than 2° C. below the critical temperature. The exact temperature is affected by a number of circumstances: thus it is lower with a wide capillary tube than a narrow one, and for the same capillary tube it is also lower when this tube is deeply immersed

in the liquid. Continuing the heating, the meniscus (*c*) in the capillary is seen below that (C) in the external tube (fig. 4). It then gradually loses its concavity, becomes successively plane and less defined, and frequently presents a more or less convex appearance before finally vanishing. Meanwhile the liquid in the external tube expands and undergoes the same changes, the defined surface appearing more or less convex, and then becoming black and ill-defined. It first ceases to be visible in the capillary tube. The curve fig. 5, Plate III., shows the changes in the height of liquid sulphurous anhydride in a capillary tube of 0·4358 millim. diameter in an external tube 3·2 millims. wide. The abscissæ are in degrees centigrade, the ordinates in millimetres. The measurements near the critical temperature could only be made with difficulty, and are consequently less reliable than those made at a lower temperature. Measurements of the *depression* of the liquid, or even of the diameter of the capillary tube in which the depression is observed, possess but little value, other conditions (*e. g.* rate of heating, &c.), incapable of exact expression, exercising too great an influence upon it. When the tube is rapidly cooled a local cloud of very fine particles suddenly makes its appearance, in the middle of which the liquid first appears as a fine horizontal line (fig. 4). In the capillary tube this cloud extends to a considerably greater distance, both upwards and downwards, than in the external tube. Probably this is connected with a surface-action, to be described subsequently. The mean position of the cloud is nearly that at which the tube-contents disappeared on heating. The formation of this cloud reminds one forcibly of the sudden crystallization of a supersaturated solution. The volume which the liquid then first occcupies is less than that which is possessed before its disappearance, and is considerably less if the black ill-defined surface visible on heating be taken into consideration. A persistent tendency on the part of the ether and sulphurous anhydride gases to condense at a lower temperature (particularly when somewhat rapidly cooled) than that at which they were formed is very noticeable *. Rapidly cooled (especially in narrow external tubes) the liquid, when it first becomes visible on condensation, is either level in the capillary and external tubes, or higher in those tubes in which the depression on rapid heating has been the greatest. When

* Although it is premature to draw any definite conclusion from this, it may be of interest to recall the researches of Magnus (Pogg. *Ann.* xxxviii. 592), and Regnault (*Mémoires de l'Académie* 1862, xxvi. 715–749, 335–664) on the vapour-pressures of liquid mixtures, which seem to bear a possible relation to it. In this connexion see also Cailletet's paper (Phil. Mag. March 1880, p. 235).

slowly cooled, or even when somewhat rapidly cooled in an external tube of large diameter, the liquid condenses in greater quantity in the external tube than in the capillary tube, and hence the liquid has to rise in the capillary tube before it reaches the level of the liquid outside. This is easily seen to be the case from the shape of the condensing cloud in an external tube 20 millims. in diameter. It will be subsequently seen that the resistance which the narrow tube offers to the flow of the liquid through it, is at least very closely connected with the cause of the depression *.

When the tubes are very slowly heated in the apparatus already described, the above phenomena are considerably modified. The volume occupied by the same liquid before its vaporization is then seen to be far greater than that occupied by it when rapidly heated. To illustrate this the volume of ether in a certain tube at the ordinary temperature was roughly 25. Rapidly heated, the volume at which the *liquid* disappeared was between 35 and 40; and at a volume of about 45 the last traces of black cloud had gone and the contents of the tube appeared perfectly homogeneous. Slowly heated when the liquid had expanded to a volume of 52, the meniscus was slightly, but perceptibly, concave, and disturbed by a rising bubble of gas. The enclosed capillary tube was not long; and the liquid expanded until it reached the top, and then poured into the tube and filled up the existing depression. Indications that a higher temperature is required for this increased expansion of the liquid have been observed; but whether the temperature at which the liquid disappears is the same for rapid and slow heating has not yet been satisfactorily determined. Continuing to heat, the liquid expands, and the surface is reduced to a thin and often waving line, obliterated by further expansion, or lost amongst the frequent striæ. Under these circumstances the volumes of disappearance and reappearance are nearly equal, and the corresponding temperatures sometimes differ by less than 0°·1 C. A very slight sudden rise of temperature, when the liquid has expanded beyond the volume at which it disappears under rapid heating, suffices to replace the defined surface by the black ill-defined one before described; but, apparently, when this expansion has proceeded too far, the surface then becomes broad and ill-defined, but not black. It then passes from the liquid through this ill-defined state without expansion; and the liquid in a rather wide capillary tube is then seen to be level with that outside it. Slowly cooled, the tube frequently becomes uniformly filled with striæ,

* For this idea I am indebted to a suggestion made to me by my riend Mr. Eagles.

and the light transmitted through the experimental tube by the paraffin lamp before described is observed to become gradually redder and redder. This is succeeded by the formation of a whitish incipient cloud, which finally precipitates in visible particles, often throughout the whole tube. The liquid contracts from the first moment of its condensation until it regains its original volume. Slow and regular cooling seems more difficult to attain than the corresponding conditions on heating.

When the external tube contains rather less ether than the above—that is, when about one third filled, and rapidly heated, the liquid expands and passes into gas in the usual way. Very slowly heated the liquid also expands; but after reaching a certain maximum volume, it very gradually diminishes and evaporates away. If the volume of liquid at its point of maximum expansion happens to be such that the liquid in the external and capillary tubes are almost level, the meniscus in the capillary tube shows a slight tendency to increase its height above the surface when the contraction commences. A slight sudden rise of temperature produces a result similar to that described in the last case. On cooling, the liquid in such a tube undergoes a momentary and almost inappreciable increase of volume just after condensation, and then contracts to its initial volume as in the previously described tubes. If a tube contains still less ether than the last mentioned, the liquid undergoes a more marked increase of volume when it first condenses; and this continues longer before the normal contraction sets in.

Very near the critical temperature the density of liquid and gas are almost equal; and hence it is that the meniscus in the capillary tube may remain depressed for as much as an hour and a half, although very slowly following the upward motion of that in the external tube. The meniscus in the capillary tube usually fades away a little before the surface ceases to be visible in the external tube. This may be due to its being under a slightly lower pressure.

The meniscus in a wide capillary tube is observed below the surface of the liquid in the external tube before that in a capillary of small diameter; and rapidly heated, the depression usually remains greater in the wide tube until the surface ceases to be visible. This disappearance frequently takes place first in the small, then in the larger capillary, and lastly in the external tube; when the heating is sufficiently slow, the depression becomes greatest in the tube of small diameter. Slow or rapid heating and slow cooling alike show the depression is greater in a tube roughened by hydrofluoric acid than it is in a smooth one of the same size. When the

heating is rapid, the depression in a capillary tube the immersed half of which has been previously heated and drawn out until the diameter is very small is less than it is in the corresponding tube of uniform diameter throughout. Slow heating reverses this result, as shown in fig. 3. By the mere contraction of the extreme end of the immersed part of a capillary tube the depression is almost uninfluenced. The results above described are not invariable (and some points connected with them are still the subject of inquiry); but under conditions which are apparently the most free from error they become sufficiently so to justify their being regarded as normal.

It seems probable that the effects of unequal and irregular heating are completely removed from the above experiments; for the liquid is equally depressed in two capillary tubes of equal diameter, one made of very thick, and the other of very thin glass. Rapidly heated, the depression reaches a maximum in the thick glass tube. Of two similar capillary tubes the most deeply immersed always shows the deepest depression; but the length of the tube above the liquid is apparently without influence. This led to the result described to the Society in June 1879, viz. that when the capillary tube does not dip more than a millimetre or two below the surface the liquid disappears at the same level in the capillary and external tubes.

It has been previously stated that the liquid sometimes appears convex near the critical temperature. With ether in a tube 20 millims. in diameter, this apparent convexity is so well marked that I had at first much difficulty in satisfying myself that such was not the case. By means of a lamp, cylindrical lens, and slit, a bright line of light is easily thrown into the liquid in such a tube heated in a large beaker of oil. The changes in the form of the reflected image were then observed ; and from this it was inferred that when the liquid surface appears convex and well-defined, as seen through the horizontal telescope, it is slightly but unmistakably concave, and remains so until it loses the power of reflecting when it is plane. This apparent convexity is caused by the raising of the far edge of the liquid by refraction; or perhaps it may resemble mirage, as I have distinctly seen the *surface* of the liquid in the horizontal telescope. The surface subsequently becomes black and ill-defined, and, as Professor McLeod has pointed out to me, closely resembles the surface of alcohol and water in a test-tube into which they have been carefully poured so as not to mix. The band of light which leaves the cylindrical lens, and passes through the vertical tube of ether, gives

rise to some remarkable refraction figures at the surface of the liquid. By substituting the test-tube of alcohol and water for the tube of ether, corresponding figures are obtained—heat in one case, and diffusion in the other, causing these figures to pass through the same changes. It appears possible that the black surface may be due to the mixing of the liquid ether with its vapour when they are of nearly equal density. This view receives some support from the fact that just before the defined surface of the liquid is lost, the convection-currents become so rapid and violent as to disturb and apparently almost break through it into the vapour above.

The above conclusion as to the form of the surface receives confirmation from some experiments with an external tube enclosing two glass plates 0·15 millim. thick, and 11 millims. wide, and separated from each other by a short distance. Not the least rounding of the surface of the liquid could be detected either between or around the plates. That no depression of the liquid was observed between the plates, corresponding to that in the capillary tubes, may be explained by the very small expansion of the liquid in this tube at that point at which the depression usually takes place. Under the same conditions the depression is absent also from a capillary tube. The expansion, as already stated, is dependent upon the amount of liquid in the tube and upon the rate at which it is heated. On cooling, just as the cloud appeared, and before the liquid line had made its appearance in it, an interesting action was observed taking place on the surfaces of these glass plates; for, extending downwards into the cloud and considerably above its upper limit, a liquid film could be seen running over their surfaces. This probably affords evidence of a surface-action preceptibly influencing the position at which the condensation of the liquid takes place.

It remains only to briefly notice a class of facts to which reference has not yet been made, but which includes certain conditions capable of modifying some of the results described in a part of this paper. It has been very frequently observed that when a tube is heated for the first time, the depression is smaller than it is when the tube is again heated within a short time of the first experimnnt. In a few capillary tubes the liquid is seen to disappear at the same level as the liquid outside them; but reheating shortly after the liquid has condensed, the usual depression is observed. When such a tube has been left a sufficient length of time in contact with the liquid on heating, the same result is obtained as at first; this may be repeated indefinitely. For two tubes this time has been determined, and in both cases found to be about 20 hours; a

shorter period merely sufficed to *diminish* the depression. Probably closely connected with this action is the gradual decrease Quincke * has observed that time produces in the form of a bubble of gas in a liquid, and of a drop of mercury. To this class of facts are also nearly related the decrease in the intensity of Quincke's † electrical diaphragm-currents, and that which ‡ I have shown to take place in the electromotive force produced when water is forced through capillary tubes. Elster § has recently extended the observation to a similar variation in the electromotive force set up by liquids flowing over the surfaces of solids. Dorn || has investigated at some length the cause of this action in the case of tubes, and has shown that it is capable of modification in various ways, some of which appear capable of exercising a corresponding control over the above-described depression of a liquid in a capillary tube at a temperature near the critical.

It is proposed to continue the still incomplete portions of this inquiry in a paper to the Society next session.

In conclusion I beg leave to express my thanks to Professor McLeod, not only for having advised me to extend my observations to higher temperatures than those at first employed, but also for the willingness he has always shown to aid me with valuable suggestions.

Summary of Contents.

1. When a tube enclosing a capillary tube dipping into alcohol, ether, or sulphurous anhydride is heated, the liquid sinks in the capillary, and rises by expansion in the outer tube. Between 2° and 3° C. below the critical temperatures of these liquids both surfaces become level; and on continuing to heat, the concave meniscus in the capillary tube is seen below that in the external tube. The extent of this depression depends on the diameter &c. of the capillary tube, and on the nature of its internal surface. When the end of a capillary tube dips very slightly below the surface of the liquid, it is level in the capillary and external tubes at the disappearance of the liquid.

2. In some capillary tubes the liquid is not depressed, but disappears at the level of the liquid in which they are immersed on first heating. Once heated, long contact between liquid and tube is necessary to prevent the formation of the depression

* Pogg. *Ann.* Bd. clx. S. 576. † *Ibid.* Bd. cx. S. 56.

‡ *Ibid.* 1877, S. 345.

§ *Inaugural-Dissertation über die in freien Wasserstrahlen auftretenden electromotorischen Kräfte.* Leipzig, 1879.

|| Wiedemann's *Ann.* Bd. ix. 1880, S. 523. Compare also Helmholtz, Wied. *Ann.* vii. p. 337 (1879).

on again heating. For two tubes which were examined, this time was in each case about 20 hours ; a shorter period merely sufficed to *diminish* the depresion. The depression is the result of an action between the liquid and the inner glass surface of the capillary tube.

3. Indications that surfaces exercise a slight action in determining the position at which the liquid condenses in the external tube have been observed.

4. By reflecting a bright line of light from the apparently convex and well-defined surface of ether in a tube of 20 millims. diameter at a temperature near the critical, it may be inferred to remain concave until it loses the power of reflecting when it is plane. The apparent convexity is the result of refraction, or, perhaps, of an action resembling mirage.

5. The black ill-defined band which immediately succeeds the disappearance of the liquid surface is the result of too rapid heating, and possibly due to the mixing of liquid and vapour when they are of nearly equal density. When very slowly heated, as described, the defined concave surface is gradually obliterated, and is last seen as a fine and often waving line. Under this condition also the volume of the liquid at its disappearance is greater than when it is rapidly heated. When the liquid is vaporized by rapid heating, it has a higher temperature and larger volume at the time of disappearance than it has when first condensed by cooling; slowly heated and cooled, these volumes and temperatures are more nearly the same.

Royal Indian Engineering College,
June 1880.

XXIV. *Vibrations of a Columnar Vortex.* *By* Sir William Thomson*.

THIS is a case of fluid-motion, in which the stream-lines are approximately circles, with their centres in one line (the axis of the vortex) and the velocities approximately constant, and approximately equal at equal distances from the axis. As a preliminary to treating it, it is convenient to express the equations of motion of a homogeneous incompressible inviscid fluid (the description of fluid to which the present investigation is confined) in terms of "columnar coordinates," r, θ, z—that is, coordinates such that $r\cos\theta = x$, $r\sin\theta = y$.

If we call the density unity, and if we denote by $\dot{x}, \dot{y}, \dot{z}$ the velocity-components of the fluid particle which at time t is

* From the Proceedings of the Royal Society of Edinburgh, March 1, 1880.

passing through the point (x, y, z), and by $\frac{d}{dt}, \frac{d}{dx}, \frac{d}{dy}, \frac{d}{dz}$ differentiations respectively on the supposition of x, y, z constant, t, y, z constant, t, x, z constant, and t, x, y constant, the ordinary equations of motion are

$$\left.\begin{aligned} -\frac{dp}{dx} &= \frac{d\dot{x}}{dt} + \dot{x}\frac{d\dot{x}}{dx} + \dot{y}\frac{d\dot{x}}{dy} + \dot{z}\frac{d\dot{x}}{dz}, \\ -\frac{dp}{dy} &= \frac{d\dot{y}}{dt} + \dot{x}\frac{d\dot{y}}{dx} + \dot{y}\frac{d\dot{y}}{dy} + \dot{z}\frac{d\dot{y}}{dz}, \\ -\frac{dp}{dz} &= \frac{d\dot{z}}{dt} + \dot{x}\frac{d\dot{z}}{dx} + \dot{y}\frac{d\dot{z}}{dy} + \dot{z}\frac{d\dot{z}}{dz}, \end{aligned}\right\} \quad . \quad . \quad (1)$$

and

$$\frac{d\dot{x}}{dx} + \frac{d\dot{y}}{dy} + \frac{d\dot{z}}{dz} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

To transform to the columnar coordinates, we have

$$\left.\begin{aligned} x &= r\cos\theta, \quad y = r\sin\theta, \\ \dot{x} &= \dot{r}\cos\theta - r\dot{\theta}\sin\theta, \\ \dot{y} &= \dot{r}\sin\theta + r\dot{\theta}\cos\theta, \\ \frac{d}{dx} &= \cos\theta\frac{d}{dr} - \sin\theta\frac{d}{rd\theta}, \\ \frac{d}{dy} &= \sin\theta\frac{d}{dr} + \cos\theta\frac{d}{rd\theta}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The transformed equations are

$$\left.\begin{aligned} -\frac{dp}{dr} &= \frac{d\dot{r}}{dt} + \dot{r}\frac{d\dot{r}}{dr} - \frac{(r\dot{\theta})^2}{r} + \dot{\theta}\frac{d\dot{r}}{d\theta} + \dot{z}\frac{d\dot{r}}{dz}, \\ -\frac{dp}{rd\theta} &= r\frac{d\dot{\theta}}{dt} + \dot{r}\frac{d(r\dot{\theta})}{dr} + \dot{r}\dot{\theta} + \dot{\theta}\frac{d(r\dot{\theta})}{d\theta} + \dot{z}\frac{d(r\dot{\theta})}{dz}, \\ -\frac{dp}{dz} &= \frac{d\dot{z}}{dt} + \dot{r}\frac{d\dot{z}}{dr} + \dot{\theta}\frac{d\dot{z}}{d\theta} + \dot{z}\frac{d\dot{z}}{dz}, \end{aligned}\right\} \quad (4)$$

and

$$\frac{d\dot{r}}{dr} + \frac{\dot{r}}{r} + \frac{d(r\dot{\theta})}{rdr} + \frac{d\dot{z}}{dz} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Now let the motion be approximately in circles round $0z$, with velocity everywhere approximately equal to T, a function of r; and to fulfil these conditions, assume

$$\left.\begin{aligned} &\dot{r} = \rho\cos mz\sin(nt - i\theta), \quad r\dot{\theta} = \mathrm{T} + \tau\cos mz\cos(nt - i\theta), \\ &\dot{z} = w\sin mz\sin(nt - i\theta), \quad p = \mathrm{P} + \varpi\cos mz\cos(nt - i\theta), \\ &\text{with} \qquad \mathrm{P} = \int\frac{\mathrm{T}^2 dr}{r}, \end{aligned}\right\} (6)$$

where ρ, τ, w, and ϖ are functions of r, each infinitely small in comparison with T. Substituting in (4) and (5) and neglecting squares and products of the infinitely small quantities, we find

$$\left.\begin{aligned} -\frac{d\varpi}{dr} &= \left(n - i\frac{\mathrm{T}}{r}\right)\rho - 2\frac{\mathrm{T}}{r}\tau, \\ -\frac{i\varpi}{r} &= -\left(n - i\frac{\mathrm{T}}{r}\right)\tau + \left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)\rho, \\ +m\varpi &= \left(n - i\frac{\mathrm{T}}{r}\right)w, \end{aligned}\right\} \quad . \quad . \quad (7)$$

$$\frac{d\rho}{dr} + \frac{\rho}{r} + \frac{i\tau}{r} + mw = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Taking (7), eliminating ϖ, and resolving for ρ, τ, we find

$$\left.\begin{aligned} \rho &= \frac{1}{m\mathrm{D}}\left(n - i\frac{\mathrm{T}}{r}\right)\left\{\left(n - i\frac{\mathrm{T}}{r}\right)\frac{dw}{dr} - \frac{i}{r}\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)w\right\}, \\ \tau &= \frac{1}{m\mathrm{D}}\left\{\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right)\left(n - i\frac{\mathrm{T}}{r}\right)\frac{dw}{dr} + \frac{i}{r}\left[\frac{\mathrm{T}^2}{r^2} - \frac{d\mathrm{T}^2}{dr^2} - \left(n - i\frac{\mathrm{T}}{r}\right)^2\right]w\right\}, \\ \text{where} \qquad \mathrm{D} &= \frac{2\mathrm{T}}{r}\left(\frac{\mathrm{T}}{r} + \frac{d\mathrm{T}}{dr}\right) - \left(n - i\frac{\mathrm{T}}{r}\right)^2. \end{aligned}\right\} \quad . \quad (9)$$

For the particular case of $m=0$, or motion in two dimensions (r, θ), it is convenient to put

$$\frac{-w}{m} = \phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In this case the motion which superimposed on $\dot{r}=0$ and $r\dot{\theta}=\mathrm{T}$ gives the disturbed motion is irrotational, and $\phi \sin(nt - i\theta)$ is its velocity-potential. It is also to be remarked that, when m does not vanish, the superimposed motion is irrotational where, if at all, and only where $\mathrm{T} = \text{const.}/r$; and that whenever it is irrotational, ϕ, as given by (10), is its velocity-potential.

Eliminating ρ and τ from (8) by (9), we have a linear differential equation of the second order for w. The integration of this, and substitution of the result in (9), give w, ρ, and τ in terms of r, and the two arbitrary constants of integration which, with m, n, and i, are to be determined to fulfil whatever surface-conditions, or initial conditions, or conditions of maintenance are prescribed for any particular problem.

Crowds of exceedingly interesting cases present themselves. Taking one of the simplest to begin:—

Case I.

Let $\qquad \mathrm{T} = \omega r$ (ω const.), $\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad$ (11)

$$\left.\begin{array}{l}\dot{r}=c\cos mz\sin(nt-i\theta) \text{ when approximately } r=a,\\ \dot{r}=\mathfrak{c}\cos mz\sin(nt-i\theta) \quad ,, \quad ,, \quad r=\mathfrak{a},\\ c,\ \mathfrak{c},\ m,\ n,\ a,\ a' \text{ being any given quantities and } i\\ \quad\text{any given integer.}\end{array}\right\} . \quad (12)$$

The condition $T=\omega r$ simplifies (9) to

$$\left.\begin{array}{l}\rho=\dfrac{(n-i\omega)\left\{\left(n-i\omega\right)\dfrac{dw}{dr}-\dfrac{2i\omega}{r}w\right\}}{m\{4\omega^2-(n-i\omega)^2\}},\\ \tau=\dfrac{(n-i\omega)\left\{2\omega\dfrac{dw}{dr}-\dfrac{i(n-i\omega)}{r}\omega\right\}}{m\{4\omega^2-(n-i\omega)^2\}};\end{array}\right\} . \quad (13)$$

and the elimination of ρ and τ by these from (8) gives

$$-\frac{d^2w}{dr^2}+\frac{1}{r}\frac{dw}{dr}-\frac{i^2w}{r^2}+m^2\frac{4\omega^2-(n-i\omega)^2}{(n-i\omega)^2}w=0; \quad (14)$$

or

$$\left.\begin{array}{l}\dfrac{d^2w}{dr^2}+\dfrac{1}{r}\dfrac{dw}{dr}-\dfrac{i^2w}{r^2}+\nu^2w=0,\\ \nu=m\sqrt{\dfrac{4\omega^2-(n-i\omega)^2}{(n-i\omega)^2}};\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where

or

$$\left.\begin{array}{l}\dfrac{d^2w}{dr^2}+\dfrac{1}{r}\dfrac{dw}{dr}-\dfrac{i^2w}{r^2}-\sigma^2w=0,\\ \sigma=m\sqrt{\dfrac{(n-i\omega)^2-4\omega^2}{(n-i\omega)^2}}.\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where

Hence if J_i, $\mathfrak{J}_i$ denote Bessel's functions of order i, and of the first and second kinds* (that is to say, J_i finite or zero for infinitely small values of r, and $\mathfrak{J}_i$ finite or zero for infinitely great values of r), and if I_i and $\mathfrak{I}_i$ denote the corresponding real functions with ν imaginary, we have

$$w=CJ_i(\nu r)+\mathfrak{C}\mathfrak{J}_i(\nu r), \quad . \quad . \quad . \quad . \quad (17)$$

or

$$w=CI_i(\sigma r)+\mathfrak{C}\mathfrak{I}_i(\sigma r), \quad . \quad . \quad . \quad . \quad (18)$$

where C and $\mathfrak{C}$ denote arbitrary constants, to be determined in the present case by the equations of condition (12). These are equivalent to $\rho=c$ when $r=a$, and $\rho=\mathfrak{c}$ when $r=\mathfrak{a}$, and, when (16) is used for w in (13), give two simple equations to determine C and $\mathfrak{C}$.

* Compare 'Proceedings,' March 17, 1879, "Gravitational Oscillations of Rotating Water." Solution II. (Case of Circular Basins). Phil. Mag. August 1880, p. 114.

The problem thus solved is the finding of the periodic disturbance in the motion of rotating liquid in a space between two boundaries which are concentric circular cylindric when undisturbed, produced by infinitely small simple harmonic normal motion of these boundaries, distributed over them according to the simple harmonic law in respect to the coordinates z, θ. The most interesting Subcase is had by supposing the inner boundary evanescent ($\mathfrak{a}=0$), and the liquid continuous and undisturbed throughout the space contained by the outer cylindric boundary of radius a. This, as is easily seen, makes $w=0$ when $r=0$, except for the case $i=1$, and essentially, without exception, requires that $\mathfrak{C}$ be zero. Thus the solution for w becomes

$$w=\mathrm{CJ}_i(\nu r), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

or

$$w=\mathrm{CI}_i(\sigma r); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and the condition $\rho=c$ when $r=a$ gives, by (13),

$$\mathrm{C}=\frac{\nu^2 m}{\nu \mathrm{J}'_i(\nu a)-\frac{2i\omega}{(n-i\omega)a}\mathrm{J}_i(\nu a)}, \quad . \quad . \quad . \quad (21)$$

or the corresponding I formula.

By summation after the manner of Fourier, we find the solution for any arbitrary distribution of the generative disturbance over the cylindric surface (or over each of the two if we do not confine ourselves to the Subcase), and for any arbitrary periodic function of the time. It is to be remarked that (6) represents an undulation travelling round the cylinder with linear velocity na/i at the surface, or angular velocity n/i throughout. To find the interior effect of a *standing* vibration produced at the surface, we must add to the solution (6), or any sum of solutions of the same type, a solution, or a sum of solutions, in all respects the same, except with $-n$ in place of n.

It is also to be remarked that great enough values of i make ν^2 negative, and therefore ν imaginary; and for such the solutions in terms of σ and the I_i, $\mathfrak{I}_i$ functions must be used.

Case II.—*Hollow Irrotational Vortex in a fixed Cylindric Tube.*

Conditions:—

$$\left.\begin{array}{ll} & \mathrm{T}=\frac{c}{r};\ \dot{r}=0 \text{ when } r=a; \\ \text{and} & \mathrm{P}+p=0 \text{ for the disturbed orbit, } r=\mathfrak{a}+\int\dot{r}_{\mathfrak{a}}dt, \end{array}\right\}(22)$$

$\mathfrak{a}$ and a being the radii of the hollow cylindric interior, or free boundary, and of the external fixed boundary, and $\dot{r}_{\mathfrak{a}}$ the value of $\dot{r}$ when r is approximately equal to $\mathfrak{a}$. The condition $T=c/r$ simplifies (9) and (14) to

$$\rho=-\frac{1}{m}\frac{dw}{dr}, \text{ and } \tau=\frac{iw}{mr}, \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$\frac{d^2w}{dr^2}+\frac{1}{r}\frac{dw}{dr}-\frac{i^2w}{r^2}-m^2w^4; \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and by (7) we have

$$\varpi=\frac{1}{m}\left(n-\frac{ic}{r^2}\right)w. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Hence

$$w=\mathrm{C}\mathrm{I}_i(mr)+\mathfrak{C}\mathfrak{I}_i(mr); \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

and the equation of condition for the fixed boundary (radial velocity zero there) gives

$$\mathrm{C}\mathrm{I}'_i(ma)+\mathfrak{C}\mathfrak{I}'_i(ma)=0. \quad . \quad . \quad . \quad . \quad . \quad (27)$$

To find the other equation of condition, we must first find an expression for the disturbance from circular figure of the free inner boundary. Let for a moment r, θ be the coordinates of one and the same particle of fluid. We shall have

$$\theta=\int\dot{\theta}dt; \text{ and } r=\int\dot{r}dt+r_0,$$

where r_0 denotes the radius of the "mean circle" of the particle's path.

Hence, to a first approximation,

$$\theta=\frac{ct}{r^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

and therefore, by (6),

$$\dot{r}=\rho\cos mz\sin\left(n-\frac{ic}{r^2}\right)t;$$

whence

$$r=r_0-\frac{\rho}{n-\frac{ic}{r^2}}\cos mz\cos(nt-i\theta). \quad . \quad (29)$$

Hence the equation of the free boundary is

$$r=\mathfrak{a}-\frac{\rho_{(r=\mathfrak{a})}}{n-i\omega}\cos(nt-i\theta), \quad . \quad . \quad . \quad . \quad (30)$$

where

$$\omega=\frac{c}{\mathfrak{a}^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

Hence at (r, θ, z) of this surface we have, from $P=\int \frac{T^2 dr}{r}$, of (6) above,

$$P=\frac{T^2}{r}(r-\text{a})$$

$$=-\frac{c^2}{\text{a}^3}\frac{\rho(r-\text{a})}{n-i\omega}\cos mz\cos(nt-i\theta). \quad . \quad (32)$$

Hence, and by (6), and (26), and (25), and (23), the condition $P+p=0$ at the free boundary gives

$$\frac{c^2}{\text{a}^3}[CI'_i(m\text{a})+\mathfrak{C}\mathfrak{I}'_i(m\text{a})]+\frac{(n-i\omega)^2}{m}[CI_i(m\text{a})+\mathfrak{C}\mathfrak{I}_i(m\text{a})]=0. \quad . \quad . \quad . \quad . \quad (33)$$

Eliminating $C/\mathfrak{C}$ from this by (27), we get an equation to determine n, by which we find

$$n=\omega(i\pm\sqrt{N}), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

where N is an essentially positive numeric.

II.—Subcase.

A very interesting Subcase is that of $a=\infty$, which, by (27), makes $C=0$, and therefore, by (33), gives

$$N=m\text{a}\frac{-\mathfrak{I}'(m\text{a})}{\mathfrak{I}(m\text{a})}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

Whether in Case II. or Subcase II., we see that the disturbance consists of an undulation travelling round the cylinder with angular velocity

$$\omega\left(1+\frac{\sqrt{N}}{i}\right) \text{ or } \omega\left(1-\frac{\sqrt{N}}{i}\right),$$

or of two such undulations superimposed on one another, travelling round the cylinder with angular velocities greater than and (algebraically) less than the angular velocity of the mass of the liquid at its free surfaces by equal differences. The propagation of the wave of greater velocity is in the same direction as that in which the liquid revolves; the propagation of the other is in the contrary direction when $N>i^2$ (as it certainly is in some cases).

If the free surface be started in motion with one or other of the two principal angular velocities (34), or linear velocities $\text{a}\omega\left(1\pm\frac{\sqrt{N}}{i}\right)$, and the liquid be then left to itself, it will perform the simple harmonic undulatory movement represented by (6), (26), (23). But if the free surface be displaced to the corrugated form (30), and then left free either at rest or with

any other distribution of normal velocity than either of those, the corrugation will, as it were, split into two sets of waves travelling with the two different velocities $a\omega\left(1\pm\frac{\sqrt{N}}{i}\right)$.

The case $i=0$ is clearly exceptional, and can present no undulations travelling round the cylinder. It will be considered later.

The case $i=1$ is particularly important and interesting. To evaluate N for it, remark that

$$\left.\begin{aligned}&\mathrm{I}_1(mr)=\mathrm{I}'_0(mr)\\ \text{and}\quad&\mathfrak{I}_1(mr)=\mathfrak{I}'_0(mr).\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad . \quad (36)$$

Now the general solution of (24) is

$$\left.\begin{aligned}w=&\left(\mathrm{E}+\mathrm{D}\log\frac{1}{mr}\right)\left(1+\frac{m^2r^2}{2^2}+\frac{m^4r^4}{2^2.4^2}+\&c.\right)\\ &+\mathrm{D}\left(\frac{m^2r^2}{2^2}\mathrm{S}_1+\frac{m^2r^2}{2^2.4^2}\mathrm{S}_2+\&c.\right),\end{aligned}\right\}, . \ (36^*)$$

where E and D are constants. Hence, according to our notation,

$$\mathrm{I}_0(mr)=1+\frac{m^2r^2}{2^2}+\frac{m^4r^4}{2^2.4^2}+\&c.,\quad . \quad . \quad . \quad (37)$$

the constant factor being taken so as to make $\mathrm{I}_0(0)=1$.

Stokes* investigated the relation between E and D to make $w=0$ when $r=\infty$, and found it to be

$$\left.\begin{aligned}&\mathrm{E/D}=\log 8+\pi^{-\frac{1}{2}}\Gamma'\tfrac{1}{2}=+2{\cdot}079442-1{\cdot}963510={\cdot}11593;\\ &\text{or, to 20 places,}\\ &\mathrm{E/D}={\cdot}11593\ 15156\ 58412\ 44881.\quad . \quad . \quad . \quad . \quad . \quad .\end{aligned}\right\}$$

Hence, and by convenient assumption for constant factor,

$$\left.\begin{aligned}\mathfrak{I}_0(mr)=&\log\frac{1}{mr}\left(1+\frac{m^2r^2}{2^2}+\frac{m^4r^4}{2^2.4^2}+\&c.\right)\\ &+\frac{m^2r^2}{2^2}(\mathrm{S}_1+{\cdot}11593)+\frac{m^4r^4}{2^2.4^2}(\mathrm{S}_2+{\cdot}11593)+\&c.\end{aligned}\right\}\ (39)$$

It is to be remarked that the series in (36) and (39) are convergent, however great be mr; though for values of mr

* "On the Effect of Internal Friction on the Motion of Pendulums," equations (93) and (106). (Camb. Phil. Trans. Dec. 1850.)

P.S.—I am informed by Mr. J. W. L. Glaisher that Gauss, in section 32 of his "Disquisitiones Generales circa seriem infinitam $1+\frac{\alpha.\beta}{1.\gamma}x+$ &c.," (*Opera*, vol. iii. p. 155), gives the value of $-\pi^{-\frac{1}{2}}\Gamma'\frac{1}{2}$, or $-\psi(-\frac{1}{2})$, in his

exceeding 6 or 7 the semiconvergent expressions* will give the values of the functions nearly enough for most practical purposes, with much less arithmetical labour.

From (37) and (39) we find, by differentiation,

$$\left.\begin{aligned} \mathrm{I}_1(mr) &= \frac{mr}{2} + \frac{m^3r^3}{2^2.4} + \frac{m^5r^5}{2^2.4^2.6} + \&c., \\ \mathrm{I}'_1(mr) &= \frac{1}{2} + \frac{3m^2r^2}{2^2.4} + \frac{5m^4r^4}{2^2.4^2.6} + \&c. \end{aligned}\right\} \quad . \quad . \quad (40)$$

$$\left.\begin{aligned} \mathfrak{K}_1(mr) = {} & \frac{1}{mr} - \frac{mr}{2^2}[-1+2(\mathrm{S}_1+\cdot1159315)] \\ & + \frac{m^3r^3}{2^2.4^2}[-1+2(\mathrm{S}_2+\cdot1159315)] + \&c. \\ & - \log\frac{1}{mr}\left(\frac{mr}{2} + \frac{m^3r^3}{2^2.4} + \frac{m^5r^5}{2^2.4^26} + \&c\right), \\ \mathfrak{K}'_1(mr) = {} & \frac{-1}{m^2r^2} - \frac{1}{2^2}[-3+2(\mathrm{S}_1+\cdot1159315)] \\ & + \frac{m^2r^2}{2^2.4^2}[7-6(\mathrm{S}_2-\cdot1159315)] + \&c. \\ & - \log\frac{1}{mr}\left(\frac{1}{2} + \frac{3m^2r^2}{2^2.4} + \frac{5m^4r^4}{2^2.4^2.6} + \&c.\right). \end{aligned}\right\} \quad (41)$$

For an illustration of Case II. with $i=1$, suppose ma to be very small. Remarking that $\mathrm{S}_1=1$, we have

$$\mathrm{N} = \frac{-ma\mathfrak{K}'_1(ma)}{\mathfrak{K}_1(ma)} = \frac{1+\dfrac{m^2a^2}{2}\left[\log\dfrac{1}{m} - \frac{1}{2} + \cdot1159\right]}{1-\dfrac{m^2a^2}{2}\left[\log\dfrac{1}{ma} + \frac{1}{2} + \cdot1159\right]}$$

$$= 1 + m^2a^2\left(\log\frac{1}{ma} + \cdot1159\right). \quad . \quad (42)$$

Hence in this case, at all events, $\mathrm{N} > i^2$; and the angular velocity of the slow wave, in the reverse direction to that of the

---

notation, to 23 places as follows:—

1·96351 00260 21423 47944 099.

Thus it appears that the last figure in Stokes's result (106) ought, as in the text, to be 0 instead of 2. In Callet's Tables we find

$\log_e 8 = 2{\cdot}07944\ 15416\ 79835\ 92825$;

and subtracting the former number from this, we have the value of E to 20 places given the text.

* Stokes, ibid.

liquid's revolution, is

$$-n=\tfrac{1}{2}\omega m^2 a^2\left(\log\frac{1}{ma}+\cdot 1159\right). \quad . \quad . \quad . \quad (43)$$

This is very small in comparison with

$$2\omega+\tfrac{1}{2}\omega m^2 a^2\left(\log\frac{1}{ma}+\cdot 1159\right), \quad . \quad . \quad . \quad (44)$$

the angular velocity of the direct wave; and therefore clearly, if the initial normal velocity of the surface when left free after being displaced from its cylindrical figure of equilibrium be zero or any thing small, the amplitude of the quicker direct wave will be very small in proportion to that of the reverse slow one.

Case III.

A slightly disturbed vortex column in liquid extending through all space between two parallel planes; the undisturbed column consisting of a core of uniform vorticity (that is to say, rotating like a solid), surrounded by irrotationally revolving liquid with no slip at the cylindric interface. Denoting by a the radius of this cylinder, we have

$$\left.\begin{aligned} &\mathrm{T}=\omega r \quad \text{when} \quad r<a, \\ \text{and}\quad &\mathrm{T}=\omega\frac{a^2}{r} \quad \text{,,} \quad r>a. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (45)$$

Hence (13), (14) hold for $r<a$, and (23), (24) for $r>a$.

Going back to the form of assumption (6), we see that it suits the condition of rigid boundary planes if Oz be perpendicular to them, O in one of them, and the distance between them π/m.

The conditions to be fulfilled at the interface between core and surrounding liquid are that ρ and w must have the same values on the two sides of it: it is easily proved that this implies also equal values of τ on the two sides. The equality of ρ on the two sides of the interface gives, by (13) and (23),

$$\left\{\frac{(i\omega-u)\left[(i\omega-n)\dfrac{dw}{dr}+\dfrac{2i\omega}{r}w\right]}{4\omega^2-(i\omega-n)^2}\right\}^{\text{internal}}_{r=a}=-\left(\frac{dw}{dr}\right)^{\text{external}}_{r=a}; \quad . \quad (46)$$

and from this and the equality of w on the two sides we have

$$\frac{(i\omega-n)\left[(i\omega-n)\left(\dfrac{dw}{wdr}\right)^{\text{internal}}_{r=a}+\dfrac{2i\omega}{a}\right]}{4\omega^2-(i\omega-n)^2}=-\left(\frac{dw}{wdr}\right)^{\text{external}}_{r=a}. \quad (47)$$

The condition that the liquid extends to infinity all round makes $w=0$ when $r=\infty$. Hence the proper integral of (24) is of the form $\mathfrak{K}_i$: and the condition of undisturbed continuity through the axis shows that the proper integral of (13) is of the form J_i. Hence

$$\left.\begin{array}{l} w=CJ_i(\nu r) \text{ for } r<a, \\ w=\mathfrak{C}\mathfrak{K}_i(mr) \;\;,,\;\; r>a, \end{array}\right\}, \quad . \quad . \quad . \quad . \quad (48)$$

and

by which (47) becomes

$$\frac{(i\omega-n)\left[(i\omega-n)\dfrac{\nu J'_i(\nu a)}{J_i(\nu a)}+\dfrac{2i\omega}{a}\right]}{4\omega^2-(i\omega-n)^2}=\frac{-m\mathfrak{K}'_i(ma)}{\mathfrak{K}_i(ma)}; \quad (49)$$

or by (15),

$$\frac{J'_i(q)}{qJ_i(q)}+\frac{i}{q^2\lambda}=\frac{-\mathfrak{K}'_i(ma)}{ma\mathfrak{K}_i(ma)}; \quad . \quad . \quad . \quad (50)$$

where

$$\lambda=\frac{i\omega-n}{2\omega}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

and

$$q^2=m^2a^2\frac{1-\lambda^2}{\lambda^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52$$

Remarking that $J_i(q)$ is the same for positive and negative values of q, and that it passes from positive through zero to a finite negative maximum, thence through zero to a finite positive maximum, and so on an infinite number of times, while q is increased from 0 to ∞, we see that while λ is increased from -1 to 0, the first member of (50) passes an infinite number of times continuously through all real values from $-\infty$ to $+\infty$, and that it does the same when λ is diminished from $+1$ to 0. Hence (50), regarded as a transcendental equation in λ, has an infinite number of roots between -1 and 0 and an infinite number between 0 and $+1$. And it has no roots except between -1 and $+1$, because its second member is clearly positive, whatever be ma; and its first member is essentially real and negative for all real values of λ except between -1 and $+1$, as we see by remarking that when $\lambda^2>1$ $-q^2$ is real and positive, and $-J'_i(q)/qJ_i(q)$ is real and $>i/(-q^2)$; while $i/q^2\lambda$, whether positive or negative, is of less absolute value than $i/(-q^2)$.

Each of the infinite number of values of λ yielded by (50) gives, by (51) and (13), a solution of the problem of finding simple harmonic vibrations of a columnar vortex, with m of any assumed value. All possible simple harmonic vibrations

are thus found: and summation, after the manner of Fourier, for different values of m, with different amplitudes and different epochs, gives every possible motion, deviating infinitely little from the undisturbed motion in circular orbits.

The simplest Subcase, that of $i=0$, is curiously interesting. For it (50), (51), (52) give

$$\frac{J'_0(q)}{qJ_0(q)}=\frac{-\mathfrak{J}'_0(ma)}{ma\mathfrak{J}_0(ma)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (53)$$

and

$$n=\frac{2\omega ma}{\sqrt{(m^2a^2+q^2)}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

The successive roots of (53), regarded as a transcendental equation in q, lie between the 1st, 3rd, 5th... roots of $J_0(q)=0$, in order of ascending values of q, and the next greater roots of $J'_0(q)=0$, coming nearer and nearer down to the roots of J_0 the greater they are. They are easily calculated by aid of Hansen's Tables of Bessel's functions J_0 and J_1 (which is equal to J'_0) from $q=0$ to $q=20$*. When ma is a small fraction of unity, the second member of (53) is a large number; and even the smallest root exceeds by but a small fraction the first root of $J_0(q)=0$, which, according to Hansen's Table, is 2·4049, or, approximately enough for the present, 2·4. In every case in which q is very large in comparison with ma, whether ma is small or not, (54) gives

$$n=\frac{2\omega ma}{q} \text{ approximately.}$$

Now, going back to (6), we see that the summation of two solutions to constitute waves propagated along the length of the column gives:—

$$\left.\begin{array}{ll} \dot{r}=-\rho\sin(nt-mz); & r\theta=\mathrm{T}+\tau\cos(nt-mz); \\ \dot{z}=w\cos(nt-mz); & p=+\varpi\cos(nt-mz). \end{array}\right\} \quad (55)$$

The velocity of propagation of these waves is n/m. Hence, when q is large in comparison with ma, the velocity of longitudinal waves is $2\omega a/q$, or $2/q$ of the translational velocity of the surface of the core in its circular orbit. This is $1/1{\cdot}2$, or $\frac{5}{6}$ of the translational velocity, in the case of ma small, and the *mode* corresponding to the smallest root of (53). A full examination of the internal motion of the core, as expressed by (55), (13), (48), (15) is most interesting and instructive. It must form a more developed communication to the Royal Society.

* Republished in Lömmel's *Besselsche Functionen*, Leipzig, 1868.

The Subcase of $i=1$, and ma very small, is particularly interesting and important. In it we have, by (42), for the second member of (50), approximately,

$$\frac{-\mathfrak{K}'_1(ma)}{ma\mathfrak{K}_1(ma)}=\frac{1}{m^2a^2}\left[1+m^2a^2\left(\log\frac{1}{ma}+\cdot 1159\right)\right]. \quad . \quad (56)$$

In this case the smallest root, q, is comparable with ma, and all the others are large in comparison with ma. To find the smallest, remark that when q is very small we have, to a second approximation,

$$\frac{J'_1(q)}{qJ_1(q)}=\frac{1}{q^2}-\frac{1}{4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

Hence (50), with $i=1$, becomes, to a first approximation,

$$\frac{1}{q^2}\left(1+\frac{1}{\lambda}\right)=\frac{1}{m^2a^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

This and (52), used to find the two unknowns λ and q^2, give

$$\lambda=\tfrac{1}{2}, \text{ and } q^2=3m^2a^2,$$

for a first approximation. Now, with $i=1$, (51) becomes

$$\lambda=\tfrac{1}{2}\left(1-\frac{n}{\omega}\right),$$

and therefore n/ω is infinitely small. Hence (52) gives for a second approximation,

$$q^2=3m^2a^2\left(1+\frac{8n}{3\omega}\right), \quad . \quad . \quad . \quad . \quad . \quad (59)$$

and we have

$$\frac{1}{q^2\lambda}=\frac{2}{3}\frac{1}{m^2a^2}\left(1-\frac{5n}{3\omega}\right). \quad . \quad . \quad . \quad . \quad (60)$$

Using now (57), (59), (60), and (56) in (50), we find, to a second approximation,

$$\frac{1}{3ma^2}\left(1-\frac{8n}{3\omega}\right)-\frac{1}{4}+\frac{2}{3ma^2}\left(1-\frac{5n}{3\omega}\right)$$
$$=\frac{1}{m^2a^2}\left[1+m^2a^2\left(\log\frac{1}{ma}+\cdot 1159\right)\right],$$

whence

$$\frac{-n}{\omega}=\frac{1}{2}m^2a^2\left(\log\frac{1}{ma}+\frac{1}{4}+\cdot 1159\right). \quad . \quad . \quad (61)$$

Compare this result with (43) above. The fact that, as in (43), $-n$ is positive in (61), shows that in this case also the direction in which the disturbance travels round the cylinder is

retrograde (or opposite to that of the translation of fluid in the undisturbed vortex); and, as was to be expected, the values of $-n$ are approximately equal in the two cases when ma is small enough; but it is smaller by a relatively small difference in (60) than in (43), as was also to be expected.

The case of ma small and $i>1$ has a particularly simple approximate solution for the smallest q-root of the transcendental (50). With any value of i instead of unity we still have (58), as a first approximation for q small. Eliminating q^2/m^2a^2 between this and (52), we still find $\lambda=\frac{1}{2}$; but instead of $n=0$ by (51), we now have $n=(i-1)\omega$. Thus is proved the solution for waves of deformation of sectional figure travelling round a cylindrical vortex, announced thirteen years ago without proof in my first article respecting Vortex Motion*.

XXV. *On the Diagrammatic and Mechanical Representation of Propositions and Reasonings.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

MR. VENN has kindly sent me a copy of his very interesting paper in the Philosophical Magazine for July, in which he explains a method which he has invented for solving logical problems by means of diagrams. The method is certainly ingenious, and for verifying analytical solutions of easy and elementary problems it would, I think, be useful in the hands of a teacher; but I cannot agree with its inventor's estimate of its practical utility in other respects, much less with his opinion as to its superiority over rival methods. Speaking of his diagram for five-letter problems, Mr. Venn says:—

"It must be admitted that such a diagram is not quite so simple to draw as one might wish it to be; but then we must remember what are the alternatives before any one who wishes to grapple effectively with five terms and all the thirty-two possibilities which they yield. *He must either write down, or in some way or other have set before him, all those thirty-two compounds of which* X Y Z W V *is a sample; that is, he must contemplate the array produced by* 160 *letters.*"

Fom the words in italics it is evident that Mr. Venn does not yet appreciate the advantages of my own method, which assuredly lays one under no such onerous obligation as he mentions. It grapples effectively, not merely with problems

* "Vortex Atoms," Proc. Roy. Soc. Edinb. Feb. 18, 1867.

of five terms, but with problems of six, seven, eight, or even more terms; and it does so because it does *not* oblige one to take into separate consideration all those perplexing possibilities with which Mr. Venn's and similar methods are hampered. That the readers of this Magazine may be able to judge fairly as to the respective capabilities of Mr. Venn's method and mine, I will first solve one of his four-letter problems, and then a six-letter problem of my own, which though exceedingly easy by my method, would, if I am not greatly mistaken, subject his diagrammatic method to a severe strain.

"Every X is either Y or Z; every Y is either Z or W; every Z is either W or X; and every W is either X or Y: what further condition, if any, is needed to ensure that every XY shall be W?"

This is a special case of the following more general problem:—

Given a series of implications, $A : a$, $B : b$, $C : c$, &c.; what is the weakest implication that need be added to these data to justify the inference $m : n$?

The answer is $mn' : Aa' + Bb' + Cc' + \ldots$

When A, a, B, b, &c. are complex expressions involving m or n or both, great simplification may be effected by substituting in these expressions 1 for m and n', and therefore 0 for m' and n. In Mr. Venn's problem the data are (when expressed in my notation)

$$x : y + z, \quad y : z + w, \quad z : w + x, \quad w : x + y,$$

and the weakest addition to the premises to justify the inference $xy : w$ is therefore

$$xyw' : xy'z' + yz'w' + zw'x' + wx'y'.$$

Substituting 1 for every x, y, and w' (and therefore 0 for every x', y', and w) in the consequent of this implication, the implication becomes $xyw' : z'$, which is equivalent to $xyw'z : 0$, or $xyz : w$, the result required. In actual practical working these substitutions of unity and zero would be made mentally while writing down the consequent of the required implication, so that the result may fairly be said to follow directly from mere inspection of the data.

This and the other problems given by Mr. Venn are much too easy: the following problem, involving six letters, would be a fairer test of the power of his method; and I should much like to see his solution of it.

Taking $ax + by : cd'$ as the symbolical expression of the statement "whenever the event A happens with X, or B with Y, then C happens without D," and so on for similar state-

ments; when may we infer from the four implications $ax+by : cd'$, $bx+ay : c'd$, $cx'+dy' : ab'$, and $dx'+cy' : a'b$, (1) that either X or Y has happened but not both, (2) that both X and Y have happened or else neither of them? What combinations among the events A, B, C, D are impossible?

In other words, we are required to find in terms of a, b, c, d, (1) the weakest antecedent of $x'y+xy'$, (2) the weakest antecedent of $xy+x'y'$, and (3) what combinations among the statements a, b, c, d are inconsistent with the data.

From the data

$$(ax+by : cd')(bx+ay : c'd)(cx'+dy' : ab')(dx'+cy' : a'b)$$

we get by mere inspection

$$xy : (a'b'+cd')(a'b'+c'd) :: a'b'^{*},$$

$$x'y' : (c'd'+ab')(c'd'+a'b) :: c'd'.$$

Hence

$$xy+x'y' : a'b'+c'd';$$

that is,

$$(a+b)(c+d) : x'y+xy'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Again,

$$x'y : (b'+cd')(a'+c'd)(c'+ab')(d'+a'b)$$
$$:: (a'b'+a'cd'+b'c'd)(c'd'+a'bc'+ab'd') :: a'b'c'd'.$$

From the symmetry of the conditions we thence get

$$xy' : a'b'c'd'.$$

Hence

$$x'y+xy' : a'b'c'd';$$

that is,

$$a+b+c+d : xy+x'y'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Also, since

$$xy+x'y'+x'y+xy'=1,$$

we get

$$1 : a'b'+c'd'+\underline{a'b'c'd'};$$

that is, omitting the redundant term underlined,

$$(a+b)(c+d) : 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This completes the solution. From (1) we learn that we may infer that one and one only of the events X and Y has happened, provided we know *that* A *or* B *has happened in conjunction with* C *or* D. But since (3) informs us that this conjunction is impossible, it follows that *the data are not suffi-*

* The symbol :: expresses *equivalence*; thus for $\alpha : \beta :: \gamma :: \delta$ read "α implies β, β is equivalent to γ, and γ is equivalent to δ.

cient to justify the inference. From (2) we learn that we may infer that both X and Y or neither of them have happened provided we know that any of the events A, B, C, D (one or more) has happened ; while (3) informs us that the combinations AC, AD, BC, BD are impossible.

Other inferences, besides those required, may easily be drawn. For instance, since

$$(xy)' :: x' + y' :: x'y' + xy' + x'y' : c'd',$$

we get $c + d : xy$. Similarly we get $a + b : x'y'$. The product of these two inferences implies (2).

Where is the formidable array of 6×2^6 (or 384) letters which Mr. Venn, unless I misunderstood his words, supposes the logician obliged to face as a necessary preliminary to all inference in every problem requiring six letters? Whether Dr. Boole's or Prof. Jevons's method can fairly be charged with imposing this heavy labour I am not prepared to say ; but my method certainly does not impose it.

Yours &c.

HUGH M'COLL.

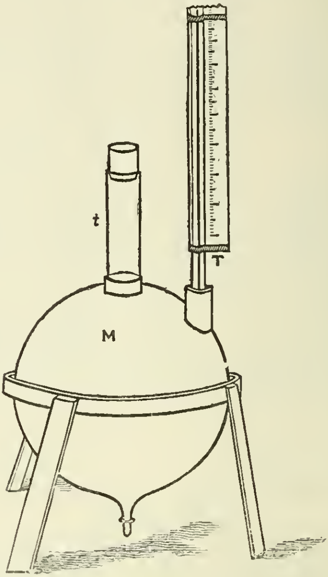
73 Rue Siblequin, Boulogne-sur-Mer,
August 3, 1880.

XXVI. *Note on a Modification of* Bunsen's *Calorimeter.* *By* W. W. GEE *and* W. STROUD*.

PROFESSOR STEWART described to the Manchester Literary and Philosophical Society, on March 4, 1879, a calorimeter devised with the purpose of obtaining the specific heat of a substance of small quantity with much readiness. It consists of a combination of part of Bunsen's arrangement with that of Favre and Silbermann. In it advantage is taken of the method employed by Bunsen—namely, that of dropping a small body, whose temperature does not differ much from that of the atmosphere, into ice-cold water contained in a small tube. In Bunsen's instrument the heat so given up by the body experimented with is measured by the change of volume produced by the melting of ice surrounding the tube. There being certain practical difficulties in the use of this method, it was thought that if the tube were surrounded by a large mass of mercury, forming the bulb of a delicate thermometer, after the manner of Favre and Silbermann, then the direct expansion of the mercury would indicate the amount of heat brought into

* Communicated by Professor Stewart to the Physical Society, having been read at the Meeting on June 26.

the tube by the body, this mercury being in its turn surrounded by melting ice not in contact with it. The instrument figured was made by Casella. T is the graduated tube of a glass thermometer whose large bulb is encased in the copper chamber M, there being only a small air space between the two. In the centre of this large bulb a test-tube *t*, provided with a cork, is tightly inserted, so that its lower portion is completely surrounded by the mercury in the bulb.



Prof. Stewart intrusted the calorimeter to us, in order that we might determine its working conditions and ascertain how far it was reliable.

The following was the way in which the apparatus was tested. The test-tube was first filled with water to the level of the mercury inside the bulb. The instrument being placed in its wooden case, the latter was then filled with pounded ice and then allowed to cool down until the column of mercury in the stem of the thermometer T was apparently stationary between two observations taken at an interval of about fifteen minutes.

After preliminary trials with various quantities of mercury dropped into the test-tube, it was found absolutely necessary to make some allowance for the loss of heat from the surface of the mercurial thermometer by radiation and convection; for although the rise produced by dropping in was almost immediate, yet during a comparatively long period of apparent maximum that followed, the heat received must have been equivalent to the heat lost by radiation and convection. (This prolonged stationary period, it was always observed, was followed by a comparatively rapid fall.) A curve of cooling, extending from $0^\circ{\cdot}3$ to $0^\circ{\cdot}0$ C., was accordingly obtained, with times as abscissæ and temperatures as ordinates. By aid of this curve the loss of heat was allowed for by a simple method of compensation.

Brass and mercury were selected for comparison. The results, after applying the correction above indicated, for

2 and 6 grams of mercury and 2 grams of brass are given below:—

2 grams	mercury	at 15°·8 C.	Corrected rise		15·6
2 „	brass	at 15°·9 C.	„	„	46·0
6 „	mercury	at 15·25 C.	„	„	48·5

An inspection of these results, knowing that the specific heat of brass is about three times that of mercury, will show that the principle of the instrument is experimentally verified. The bore of the thermometer-tube being, unfortunately, large, necessitated a small rise; greater accuracy hence could not be expected. It is hoped shortly, however, to make further experiments with a new instrument having a much smaller bore, and with other needed improvements that could not in a first instrument be foreseen as wanting.

XXVII. *On the Electric Discharge in Rarefied Gases.* *By* Dr. Eugen Goldstein*.

[Plate IV.]

Part I.

On a new Differentiation of Electric Rays.

THE object of a considerable portion of my researches is to determine the laws of that remarkable motion which radiates from the kathode in a rarefied gas, and which, being characterized by propagation in straight lines, must be placed side by side with the well-known forms of vibration which constitute light-waves and sound-waves. Hittorf has shown already that this motion (or, as he terms it, an electric ray) is terminated whenever it strikes a solid obstacle. I have found, during the past year, that with this termination by solid bodies is connected a peculiar differentiation of the rays which strike on the solid wall. This knowledge then led, further, to a satisfactory explanation of the luminous processes, often mentioned in the literature of the subject, formed on the walls of the enclosing vessel by the light from the negative pole. This excitation of light has been hitherto termed *fluorescence*, and ascribed to the high refrangibility of the rays emitted by the whole mass of gas around the negative pole. It was, moreover,

* Translated from a separate impression, communicated by the Author, from the *Monatsberichte der königlichen Akademie der Wissenschaften zu Berlin* for January 1880. Prof. Helmholtz, in presenting these researches for publication, for the prosecution of which the author had received assistance from the Royal Academy, observed that the first paper formed a part of a report presented at the meeting of Jan. 28, 1878, the second (on Electric Luminous Phenomena in Gases) part of a report presented on the 29th October, 1879.

believed to be of the same nature as the production of light, which the luminous strata of the positive discharge caused in the walls of the tube, or even through the walls upon a quinine-screen, or similar fluorescent body. My researches have shown:—

(1) *The production of light by an electric ray from the negative pole in highly rarefied gas takes place only when the ray strikes upon a solid obstacle.*

(2) *It is not the whole length of the ray which produces the light, but only the end of it furthest from the negative pole.*

Both of these statements (whose complete experimental proof cannot be here described) may be easily verified by cutting off a sharply-defined pencil of light from the mass of light from the negative pole by means of a screen with a small opening. If a fluorescent screen be caused to approach such a pencil *sideways*, no fluorescence is observed, however close the approach, whether the end of the pencil strike upon a solid wall and cause fluorescence there or not.

(3) *The cause of the production of light is to be sought in an optical action.*

This may be concluded with much probability from the identity of colour given by a series of different substances (fluor-spar, calc-spar, potash-glass, lead-glass, silver chloride, &c.) when exposed to electric radiation and to the sun's rays. It follows with greater certainty from the fact that fluorescent screens are actually excited when they are so placed in the interior of the tubes that they are protected from the rectilinear radiation of rays from the kathode, but are exposed to rectilinear radiation from those portions of the walls upon which the electric rays impinge. Such screens shine out with their own peculiar light when distant about 1 centim. from the ends of the rays, which have of course no measurable length. The molecules at the ends of the rays therefore emit rays in all directions, in the same way as the particles of a glowing body, and to points which could not be reached by the direct electric radiation. (Hence it is necessary in the experiment illustrating (1) and (2) to arrange a screen so as to intercept the rays emitted obliquely by the end molecules.)

I had already found, while occupied with the influence of the surface of the negative pole on the discharge, that when the kathode does not possess a perfectly smooth surface the inequalities are very faithfully reproduced in the light excited by the electric rays on the solid wall. Thus, for example, if a coin be employed as negative pole, there is an appearance of the head on the wall of the containing vessel.

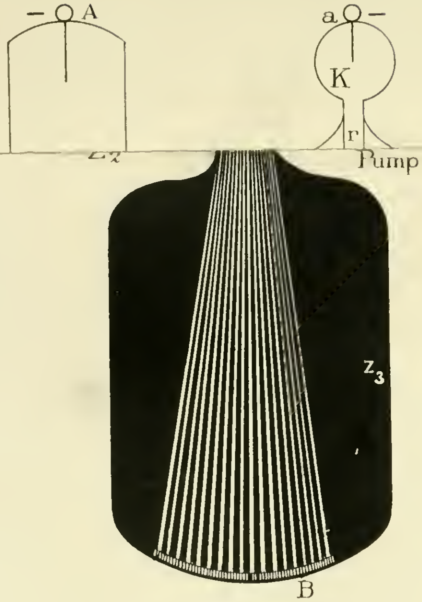


Fig. 3

Fig. 4.

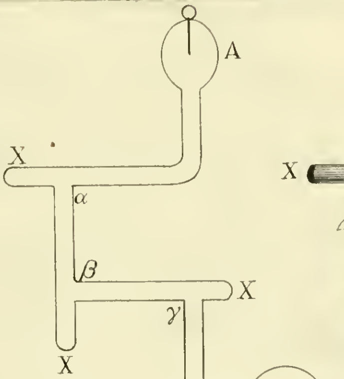
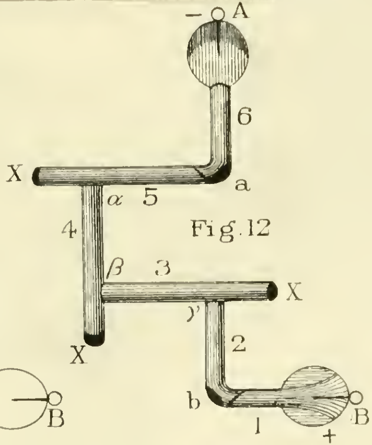


Fig. 12



Mintern Bros. lith

These and similar appearances were inexplicable as long as the production of light on the walls was ascribed to optical radiation from the whole mass of gas or from the whole length of the electric rays, which could never have produced sharp images, but only uniform illumination of the walls. On the other hand, the action of the ends of the rays as opposed to the rest of their length, which is now made clear, explains the whole of the observed phenomena at once.

The optical character of the action in question is confirmed by the existence of *photochemical action*, which, again, is exerted by the ends of the rays, and not by their whole length. Substances which are decomposed under the influence of solar rays of high refrangibility are decomposed in the same way when the ends of the rays fall upon them. I succeeded, by way of control of statements (2) and (3), in obtaining direct photographs of the image of a kathode in relief produced on the wall of the tube, by placing dry sensitive paper on the side of the tube and allowing the rays to fall on it. Pictures were obtained with potassium bichromate, silver chloride, and the highly sensitive ferric oxalate.

Further experiments showed that

(4) *The modification of the end of the ray is produced, not only when the ray impinges on a fluorescent wall, but also whenever it falls on any solid substance.*

This is seen when the electric rays are caused to fall on substances not capable of shining themselves, such as quartz, or a particular variety of mica. If then, as already described, fluorescent screens are placed at a distance from the plate of mica and ends of the rays, but exposed to any radiation from the mica, they emit light as soon as the electric rays strike the mica, although the mica itself remains dark.

If the induced current which traverses the tube be employed in the usual way (that is, without including in the circuit any other non-metallic resistance than the tube itself), this differentiation of the ends of the rays occurs only when the density is small. It may be shown, however, that

(5) *The differentiation in question is not associated with a particular pressure.* It may be produced within wide limits of density as soon as the kathode is completely surrounded with light by the interpolation in the circuit of sparks of varying length in air.

In the same way,

(6) *The phenomenon is not associated with any particular intensity of discharge.*

This is easily seen by including in one circuit several vacuum-tubes of different densities, with attention to the pre-

caution, to which I have called attention*, of isochronism in the discharge of such tubes. Observation shows that when the negative rays in one of the tubes excite the luminosity of solid bodies, this is not necessarily the case in the others; but they also exhibit the appearance as soon as they are brought to the same degree of exhaustion as the tube which first became luminous.

It follows from this that, in the modification described, the whole of the light about the kathode becomes surrounded with an exterior heterogeneous layer. The position of this new layer is dependent solely upon the position of the wall of the tube, and can be brought into any desired position at any given distance from the kathode by moving the wall to or from the kathode, while the pressure remains constant. It may even be caused to pass, still formed by the ends of the rays, through the outer layer of the kathode-light into one of the inner layers.

I am not yet in a position to explain the production of this modification of the rays.

(7) *The same differentiation occurs with the "secondary negative light,"* a name given by me to the light produced at any point of the discharge at which a contraction of the tube is introduced. A mass of light then spreads out from the point where the tube is contracted into the wider portion towards the anode, which possesses, only in a less degree, all the properties known to me of the kathode-light. The point of origin of the negative rays which here make their appearance is the last section of the narrow tube which joins the wider portion of the vessel. In the figure (Pl. IV. fig. 1) the points α are points of origin of the secondary negative light, whose rays spread out towards β. The appearance of the modified ray-endings in the case of such rays whose points of origin lie in an open space of gas, shows that the explanation of the appearance is not to be sought in the properties which the kathode possesses as solid body and metallic conductor.

(8) *The excitation of light by the ends of the negative rays is not of the same kind as the illumination called forth in the surrounding walls of the tubes by the stratification of the positive light when the rarefaction is small.*

Observation shows that the rays which excite this illumination issue from the whole mass of light; so that in passing along the column from layer to layer there is observed only uniformly diffused illumination of the wall, even when the stratifications are strongly marked and exhibit great changes in intensity of light.

* *Berl. Akad. Ber.* August 1871.

Lastly, as far as the *optical* nature of the phenomena is concerned, it cannot be doubted that, both with positive and with negative light, we have to do with a transformation of the rays of high refrangibility, whose vibrations are changed into those of greater wave-length, as is the case in the phenomena of fluorescence and phosphorescence. Previous experiments having shown me that the illumination of the solid bodies perceptibly exceeded the duration of the discharge which excited it, I have regarded the appearances observed as phenomena of phosphorescence, and not of fluorescence as previously assumed. Further, of all the numerous substances tried, not a single one was found to be transparent for these rays even in the thinnest possible layers. Neither thin films of glass, nor crystals of calc-spar and quartz, found by Mascart to be so transparent for the highly refrangible rays, allowed any trace to pass.

A glass wall, which phosphoresced brilliantly when struck directly by the ends of the rays, was covered by an excessively thin layer of collodion, by allowing a drop of ordinary collodion highly diluted with ether to run rapidly over the glass and volatilize. Even this film, whose thickness could only have amounted to a few hundredths of a millimetre, gave on the glass wall lying behind it a shadow as black as ink when the electric rays fell on it, as if it had been an opaque metallic substance.

Without being able to give numerical data, it may be concluded that the range of wave-lengths within which the vibrations of the ether are still active as light passes beyond the inferior limit assigned by Fizeau.

On the Replacement of a Kathode.

A kathode of any given form may be replaced, as far as all relationships hitherto considered are concerned, by a system of small and closely packed pores in an insulating *surface congruent with the kathode.* By way of further explanation, I will describe a tube now lying before me (fig. 2) in which a cylindrical kathode has been imitated. The tube G is composed of a bulb K, provided with an electrode *a*. The tube *r* is melted into the end of a cylinder Z of about 4 centims. diameter. A paper tube P made of stiff paper rolled together but not glued is pushed over *r*, and the other end is closed by a little glass cap *g*. The whole surface of P is covered with numerous fine needle-perforations, through which communication is made from K to the electrode *b* at the further end of Z. If now the tube is exhausted, and *a* connected with the negative and *b* with the positive pole of the induction-coil, the

discharge conveyed by the gas contained in them passes through the fine pores in the paper cylinder, which behaves qualitatively like a metallic electrode of similar shape. I have carried out the comparison of these imitation electrodes, consisting of a network of pores in an insulating material, in respect of thirteen properties independent of each other, and in all respects have found the coincidence complete. The magnetic surface of Plücker, the production of phosphorescence by the ends of the rays, the envelopment by a dark space on the side of the positive light, and so on, are all found with these imitation kathodes. Instead of paper, spun glass may be employed, and, generally, instead of an insulating substance an insulated metallic network.

These results are obtained in consequence of the phenomenon already mentioned, that the last section of a narrow tube introduced into the path of the discharge behaves as a new negative pole on the side of the anode. The light emitted by the secondary negative pole agrees also quantitatively more nearly with that from a metallic kathode, the more the section of the narrow tube differs from that of the wider tube joined to it. On the other hand, the light emitted by the secondary pole passes into positive light as soon as the section of the narrow tube is not much less than that of the part joining onto it.

One result obtained with the imitation kathodes is important —namely, that *when the sum of the small openings* of such a kathode *is equal in section to the section of the wider tube surrounding it or joining onto it, all the openings,* as far as can be observed, *produce the same action* (brightness excepted) *as if each were present alone.* The effects of the discharge are then dependent *upon the magnitude of the separate openings, and not upon the total section of the discharge.*

When the pores of the imitation kathodes, of other materials than paper, were made smaller than was possible with paper, the phenomena agreed so remarkably with those obtained with actual metallic electrodes, even to the exact colour of the discharge, that more than once it was necessary to take the tubes apart and put them together again with greater care, to be convinced that the phenomena were really due to the action of pores and not of metallic poles.

On the Mode of Discharge in Rarefied Gases.

If we have (1) a discharge-tube in which the terminal wire *b* represents the anode, and the flat electrode *a* (which occupies the section of the tube at the other end) represents the kathode,

* *Berl. Akad. Ber.* 1876, p. 280.

it is easy to assume (in accordance with the general belief) that the electricity (following the negative current) starting from *a* traverses the negative light, then passes into the first layer of the positive light, from that into the second, and so on till it reaches the anode.

But (2) let the kathode *a* be a flat surface (a strip of metal for example) whose plane is at right angles to the axis of the cylinder, but whose sides both lie in free gas. With this arrangement *a* sends out rays towards the remote side of the anode *b* exactly in the same way as in the line leading directly to *b*. The rays radiating from *a* are just as rectilinear, at right angles to the surface *a*, and without curvature, as the rays directed immediately towards *b*, and then expand as the exhaustion proceeds further and further into the space filled with gas in the direction turned away from the anode.

(3) Fig. 3 represents another case. *a* is a surface which does not completely occupy the section of the tube, but leaves room for the anode *b* beside it.

Then the rays of the negative light do not pass over to the neighbouring anode, but the negative light, as represented in the figure, spreads itself out, without reference to the position of the anode, in rectilinear rays through the whole length of the tube without any visible connexion with the anode.

How now, in the cases represented by (2) and (3), does the electricity pass from the one pole to the other? In what path is the electric excitement here propagated? The rays of the negative light, as Hittorf recognized, are electric currents, not simply a glow which surrounds the path of the actual discharge. This is shown by the behaviour of the rays towards the magnet, which is in complete accordance with the laws of Biot, Savart, and Ampère. We are therefore compelled to assume that the rays of light point out to us the path of the electricity, which consequently pursues from the kathode the path to the end of the negative rays. If, then, the current, whether we regard it as consisting in the transport of certain similar electric particles, or only in the propagation of an excitement from molecule to molecule, is to reach the anode, it must in (3) return by the path it came. And in (2), though the previous assumption of direct transference may suffice for the rays directed towards *b*, we must assume a passage of electricity to and fro for the quite similar rays turned away from the anode.

There is, however, no *action* of this hypothetical return-current to be observed. The magnet diverts the electric rays only in the manner required by the current flowing from the kathode towards the ends of the rays. The (hypothetical)

return current does not cause the least appearance of light, although it is present in the same medium, and certainly not of greater section than the direct current which fills the whole width of the tube. Any manifestation of light due to it must become visible when the direct current is diverted by the action of the magnet to one side of the tube. In the space thus rendered free, any possible luminous effect of the return stream would show itself. Experiment proves, however, that this space is *dark*.

(4) Let the kathode *a* be again a plane, whose direction is parallel to and contains the axis of the cylinder. Then the negative rays, as is always the case, are almost entirely at right angles to the radiating surface, and directed towards the sides of the tube. When the pressure is somewhat high, the rays end in free space before they reach the wall; but with smaller densities, as soon as they strike on the solid wall.

The phenomena are altogether correspondent when (5) the kathode, as is most frequently the case, is formed by a wire in the axis of the tube. Here also the rays are directed towards the sides of the tube, and in a particular case are radial in each section of the cylinder.

The electricity must therefore first traverse the direction of the negative rays to their termination, and then take a path *at right angles to it* in order to reach the anode; whilst, again, both positive and negative light have the same properties as in the preceding cases, when we assumed either direct transference or a direct and return current.

The complexity of the assumptions necessary in supposing that the current (following always the direction of the *negative* current) propagates itself from the negative light into the first stratum of the positive, from this into the second, and so on till the anode is reached, becomes still greater when account is taken of the existence of the dark space between the positive and negative light.

In the preceding cases the dark space has not been mentioned; it invariably disappears at a certain exhaustion; and for the sake of simplicity I have described the phenomena corresponding to that exhaustion.

If the kathode is again a plane (*a*) at right angles to the axis of the cylinder, the anode an electrode (*b*) of any form at the opposite end of the tube, the phenomena of discharge *when the dark space is present* correspond to fig. 4*.

* The variously coloured layers of the negative light are represented in the figures by different shading: the layer nearest the kathode is chamois-yellow, the next sky-blue; and the third, which forms the chief portion of the light, is blue with a shade of violet. Between the layers of positive

The dark space does not represent, as has been often assumed, the prolongation of the negative rays which have lost in brightness in consequence of their expansion. The negative rays have the property of rectilinear radiation and are terminated by a solid wall; they cannot, therefore, turn a corner. The appearances obtained with the discharge in bent tubes, such as figs. 5 and 6, require therefore no further explanation to show that the dark space cannot be considered as a continuation of the kathode-light, and that it possesses itself no property of rectilinear radiation.

If, therefore, we assume that the current of the kathode-light propagates itself to the first positive layer, it is necessary to suppose that the current between the two is changed for a certain distance into a new form.

Returning to the straight tube (fig. 4), if we continue the exhaustion further from the point at which the dark space makes its appearance, the positive layers retreat slowly towards the anode; at the same time the rays of the kathode-light lengthen, and more rapidly than the positive layers retreat. We thus reach a point at which the dark space disappears, and the negative light reaches to the first layer of the positive light.

Now we should be obliged to assume that the new form of conduction has disappeared, although in the visible portion of the discharge (neglecting the small displacements of the positive layers) no change has occurred in the meantime except that the negative layers have lengthened; their properties, like those of the positive layers, are altogether the same as before.

If now we exhaust still further, the positive layers retreat still further; the rays of the kathode-light lengthen still more, and again more rapidly than the positive layers retreat. The negative light *now penetrates into the layers*, whilst its properties remain unaltered, *without* confusion with those of the positive light into which it penetrates.

The proof of the penetration of the negative light into the positive light may be obtained in various ways. In fig. 7, which represents the section of a vessel consisting of three cylinders, the kathode *a* is the section of a thick wire covered on the sides with glass melted round it. If the wire *c* near *a* is made the anode, there is no manifestion of positive light except in the immediate neighbourhood of the anode, and only

light and the wall of the tube is a dark space, in wide tubes of several millimetres breadth, not mentioned in previous descriptions. In order not to make the figure of inconvenient length, the third layer of the kathode-light in fig. 4 is shown of less thickness than is actually the case.

at extremely small densities. But the kathode-light extends through the whole vessel, without being influenced by the proximity of the anode, so far as rectilinear rays radiating from *a* can reach. A bundle of rays penetrates into the widest of the three cylinders (Z_3), whose diameter is determined by that of the communicating opening between the cylinders. When the exhaustion is continued, the bundle penetrates as far as the wall B, and the ends of the rays excite there bright green phosphorescence in the form of a circular disk, which is the section of the bundle of rays by the wall B. If now *c* be disconnected from the induction-coil, and the electrode *b* in the second be made the anode while *a* remains the kathode, then there appears, as represented in the figure, a long stratified column of positive light, beginning several centimetres from the mouth of Z_1, completely filling Z_2, and reaching to the anode *b*. Z_3 remains, as before, free from positive light; but the bundle of blue negative light and phosphorescent surface on B remain visible as before, affording a decisive proof that the kathode-light penetrates into and through the positive light.

The green disk disappears as soon as *c* or *b* instead of *a* is made the kathode—when, in short, any electrode is made kathode whose rays have some other direction than those issuing from *a*. The quantitative differences which the positive and negative light show remain when they mix, as if in the space which they together fill each had separate and independent existence.

The assumption that the discharge from the negative light propagates itself into the positive layer next the negative pole, then into the second layer, and so on, necessitates the further assumption that the discharge in the last-considered phase, after it has traversed the negative light to its end (in the positive light), leaps back again to form the first layer of the positive light, and then to traverse a second time as positive light the space already once traversed as negative light under the same conditions. But even with this the complication is not yet exhausted of new assumptions to which this representation of the discharge, at the first sight so simple, leads. I have convinced myself that even the secondary negative light which radiates towards the anode from each contracted portion of the tube penetrates into the positive light which follows after the contraction; we should therefore have in each tube the leaping-back of the electricity, and its passage, once as positive light and once as negative light, as many times as the tube possesses contracted points.

If, now, we have again, as in fig. 8, a plane at right angles

to the axis of the cylinder as kathode, from which, therefore, the kathode-rays radiate in the direction of the length of the cylinder, inasmuch as with a sufficient exhaustion they would extend also through the cylinder II., we should have the following course of the electricity:—first from a to the end of the kathode-rays reaching far into II.; then backwards to the beginning of the bundle of secondary negative light at r; then forwards again (towards the anode) in its rays; and from the ends of the rays which penetrate into the positive light once more backwards to the first positive layer, so as from these to describe the same course for the third time.

But now the secondary negative light passes continuously into a layer of positive light when the section of the contraction approaches the width of the communicating portion of the vessel on the side of the anode; and special experiments show that with small densities the layers which spread out into each other are longer than their apparent intervals. It is not necessary to explain how the complication of the assumptions necessary to the ordinary representation of the discharge is thus increased.

I do not believe that the common view of the appearances thus far described, whose enumeration might be much extended, will be held to be very probable, and that in order to preserve this view, half a dozen new assumptions will be made as to invisible actions, whose reality can be shown by no verifiable result. In particular, the generally adopted convective view of the discharge ought to find a decisive contradiction in the experiments on the mutual penetration of the different portions of the discharge.

By numerous comparisons, and taking account of all apparently essential phenomena, I have been led to the following view:—

The kathode-light, each bundle of secondary negative light, as well as each layer of positive light, represent each a separate current by itself, which begins at the part of each structure turned towards the kathode, and ends at the end of the negative rays or of the stratified structure, *without the current flowing in one structure propagating itself into the next*, without the electricity which flows through one also traversing the rest in order.

I suspect, then, that *as many new points of departure of the discharge* are present in a length of gas between two electrodes as this shows of *secondary negative bundles or layers*—that as, according to experiments repeatedly mentioned, all the properties and actions of the discharge at the kathode are found again at the secondary negative light and with each layer of positive light, the intimate action is the same with these as it is with those.

This view, then, as I will briefly show, resolves all the former difficulties, and makes the whole of the hypotheses previously required unnecessary. The assumption made does not, however, merely present a simple consistent representation of the numerous phenomena which immediately lead to it; but there are, further, a large number of phenomena which harmonize with it extremely well, some of which indeed make it appear not only admissible but even necessary.

Since, according to oft-quoted experiments, the positive light is nothing else than an envelope of the negative, I shall speak of rays of electric light also when referring to the positive light, and understand by it luminous particles lying in a line which represents the direction of propagation from any point on the surface of the layer nearest the negative pole to the other bounding surface of the layer. The following proposition may be deduced from my experiments:—

The properties which the discharge shows at any point of its path are not dependent on the relationships of the point itself, so much as upon the relationships of the point from which the ray passing through it takes its origin. Or, somewhat differently expressed:—An electric ray possesses throughout its entire length the properties which the discharge possesses at its *point of origin*, and which are conditioned by the nature of this point of origin.

If, for example, two electric rays pass in equally wide similarly shaped portions of the same discharge-tube, and so in media of identical chemical and physical nature, their properties are different if the origin of one of them lies within the portion of the tube under consideration, and that of the other at the point of junction of this portion and another of less width.

It will be understood, from the example already given, that this is the explanation of all the phenomena of the influence of the section of the discharge on the character of the discharge as positive or negative. I will endeavour to make the proposition stated plain by means of a striking example.

In wide tubes filled with air (for example, cylinders of 2 centims. or more in width) the stratified positive light has a yellowish-red colour, and when analyzed with the spectroscope gives the spectrum of nitrogen described and figured by Plücker and Hittorf, consisting of numerous closely-placed bright bands.

Narrow cylinders, on the other hand, show with the same pressure a blue light, whose spectrum contains only a few of the bands seen in the spectrum of the yellowish-red light.

If, now, two wide cylinders be connected by a narrow tube

of about $1\frac{1}{2}$ millim. diameter, as in fig. 9, all the positive layers in both cylinders are yellowish-red, and the light of the narrow tube is blue. But from the end of the narrow tube turned towards the anode secondary negative light radiates into the wide cylinder, whose rays in the prolongation of the narrow tube show precisely the same blue colour and the same spectrum as the mass of the light of the narrow tube from the end of which it springs.

If with increasing exhaustion the rays of secondary negative light lengthen, the prolongation shows always the same blue colour; and so blue light with its peculiar spectrum may appear at each part of the tube previously occupied by the yellowish-red light to which the secondary negative rays extend. The neighbouring first positive layer shows yellowish-red light.

If *several* equally wide cylinders, lying in a straight line, are connected by narrow tubes of varying width each projecting into the wide cylinders, the blue which the narrow tubes show when the density is small possesses varying depth according to the width of the tube, whilst yellowish-red mixes with the blue with increasing width. From each narrow tube into the wide tube joining it on the side of the anode there issues a complex formation of secondary negative light, the middle portion of which in the prolongation of the narrow tube possesses exactly the blue (throughout its entire length, which increases with the exhaustion of the tube) corresponding to the narrow tube from which the secondary negative rays issue. On the other hand the positive layers in all the cylinders show precisely the *same* yellowish-red colour.

It must be confessed that these, with numerous similar phenomena, produce the impression that each secondary negative bundle represents a *motion which, excited at the point of origin of the bundle, is transferred to the surrounding medium*; hence each particle affected, as far as the excitation is propagated, assumes the characteristic form of motion which is produced at the *point of origin* of the rays; whilst a comparison of the discharge at any point with conduction in metals and electrolytes can afford a guide only for the relationships at the point itself.

The narrower the tubes interposed between the wider tubes are made, the purer, as already mentioned, does the blue become, and the more nearly do all the bands in its spectrum disappear with exception of the four definite bands, in which all the light is concentrated.

It is now intelligible why in a *uniformly wide tube*, of which the positive light is throughout yellowish-red, *the kathode is surrounded by blue light.*

We have seen that a kathode may be regarded as a system of fine conducting pores in a non-conducting surface. The kathode light must then consist of rays which possess the properties of the light of very narrow tubes; and in fact not only does the colour of the kathode-rays agree with the blue of narrow tubes, but the spectrum of the kathode-light consists precisely of the *same* four bands, with the same maxima of light, as that of the blue of the narrow tubes.

The suspicion expressed on p. 183, as to the true character of an apparently simple discharge between two metallic electrodes appears to be supported by the mode of *action of the magnet upon the discharge.* It follows, in fact, from it that each negative bundle, as well as each positive layer, constitutes *a whole in itself.*

Each negative bundle, in fact, kathode-light as well as secondary negative light, as well as each separate layer of positive light, upon being magnetized rolls itself together to a *single* magnetic curve by itself, altogether independently of the extent occupied in the unmagnetized condition by the bundles and layers. A negative bundle 30 centims. long forms only a single magnetic curve, just the same as a layer of 2 millims. length.*

In the same way the bundle issuing from a given point, which when of a certain length forms a single curve, when by exhaustion it is brought to a length three times, five times, or ten times as great, gives always only a single curve, inasmuch as the bundle, for example, in the equatorial position with reference to the magnet rolls itself together from the ends of

* The magnetic curves formed from the *positive* light may be clearly distinguished for a considerable distance in the neighbourhood of the kathode, and in the neighbourhood of secondary negative poles, as in fig. 9.

That they are not to be perceived in like manner in the remainder of the deflected positive light is to be explained, as I have remarked in the *Monatsber. d. Akad.* 1876, p. 282, by the curvature of the wall of the vessels commonly employed. The disturbing forces exerted by the magnet drive the discharge, and consequently the magnetic curves composed of its layers, towards the wall of the vessel. If this is in the plane of the magnetic curve, more strongly curved than the curve itself, and in the same direction, so that the wall is cut by the curve, only so much of the magnetic curve can be visible as lies in free gas-space between the two points of section. In consequence of this limitation by the wall, each magnetic curve driven to the wall is more or less nearly reduced to a point.

The sum of the light-points belonging to successive curves gives that narrow line in which the magnetized positive light appears for the greater portion of its course. This line, hitherto regarded as a single portion of the current deflected from its course while both ends are fixed, is rather to be regarded as *a succession of short magnetic curves.*

its rays; and inasmuch as the coils embrace parts of the rays lying nearer and nearer the *point of origin of the rays*, the whole length of the rays becomes ultimately compressed into a single magnetic curve.

In exactly the same way the positive layers, which indeed represent bundles of secondary negative light issuing from a tube into one less by an indefinitely small amount, unroll themthemselves from their ends turned towards the anode to the point which, in the view we have taken, is to be regarded as their point of origin—that is to say, the boundary of the layer turned towards the negative pole.

This boundary need not always preserve the same fixed position under varied experimental conditions. Nevertheless the rays always unroll themselves towards the place which happens to be their point of origin.

The appearance is very characteristic when the kathode-light in the unmagnetized condition penetrates beyond the first layer deep into the positive light. The end of the kathode-light lies then further from the kathode than the end of the first (and, according to the rarefaction, of the second, and so on) positive layer. Nevertheless the end of the kathode-light rolls itself up to the kathode into the magnetic curve which passes through it, and separated from it by a dark space; then follows on the side of the anode a curve in which are combined all the rays of the first positive layer, then a curve of the second, and so on.

This shows that it is *not the absolute position and expansion of the rays which determines the position they take when magnetized*, but the intimate relationship which exists between all the points of a ray and its *point of origin*, in consequence of which *each luminous body springing from a given point appears as a single coherent whole.*

According to this representation the consecutive layers of a discharge do not follow on into each other, even when, in consequence of the lengthening of their rays, each borders closely on the next or even partially covers it. When, then, each separate layer becomes combined into a single curve, these curves must in general be distinct, and not run together into a connected surface of light, as would be the case if there were a continuous current from each into the next.

In fact we observe that when the magnet has combined the layers into magnetic curves, the *curves* appear *separated*, there being a *dark space* between each curve and the following one.

Only when the exhaustion is so great, and the space in which the discharge takes place so narrow, that the light disappears

even before complete magnetization, does the magnetized light show no clear separation of the curves in the part of the tube under consideration.

It appears to me that the assumption of the existence of many independent currents in the discharge between the electrodes finds the strongest support from observation of the *special form* of the magnetic action on the electric rays. The mode of this action was discovered and explained for the kathode-rays by Hittorf*. My researches have shown that the results obtained by Hittorf, in opposition to the view held since Plücker's researches, hold also for each separate positive layer, in harmony with the often-repeated statement that each layer is to be regarded as a modified formation of negative light.

Let us now assume that the discharge actually forms only a single current from anode to kathode. Then the magnet must act upon the discharge, in, for example, the equatorial position, as upon an expansible flexible conductor fixed at its ends (here the electrodes), traversed in the same position by a correspondingly-directed current. The form of the magnetized column of light would then be an arc extending from the one electrode to the other in the equatorial plane; but the current could never form itself into a magnetic curve. But if the magnet acted upon a conductor fixed at one end but free to move at the other, the motion of *such* a conductor would accurately correspond to that of a magnetized kathode-ray or ray of a positive layer; and a bundle of such linear conductors radiating from one point, and all free at their other ends would, when magnetized, accurately represent the form of a bundle of the kathode-light issuing from a point.

The magnetic curve into which such a bundle rolls itself together results (according to Hittorf's investigations, which after frequent repetition, I can confirm) in the following manner:—The bundle consists of a cone of diverging rays. The rays situated near to the axis of the cone are clearly distinguished from those lying further away by their greater brightness; if therefore the axis of the bundle is placed accurately equatorially, the motion of the rays toward the magnet can be recognized in the bright centre bundle.

The bundle passes, as the strength of the magnetic action increases, from a straight thread of light into an excessively narrow flat spiral, whose plane coincides with the equatorial plane. When the magnet is powerful, the diameter of the spiral does not exceed 1 millim., so that it appears nearly as a *point of light*. If, however, the axis of the cone is inclined

* Pogg. *Ann.* cxxxvi. p. 213.

to the equatorial plane, the deformations of the bright centre bundle show the action of the magnet on those rays which make larger angles with the equatorial plane. Such an oblique bundle rolls itself, when magnetized, into a helix whose coils are steeper the greater the angle made by the rays with the equatorial plane, and the closer the nearer they lie to the magnetic pole.

With increasing strength of magnetism, the coils of these helices, of which the before-mentioned spiral forms a special case, place themselves more closely about the magnetic curve which passes through the point of origin of the rays, and change into it as far as the eye can see. Strictly, then, the magnetic curve is only the geometrical axis of the true form of magnetized light.

We see, from what has been advanced, that the forms of the magnetized rays are those which a linear conductor, free at one end and fixed at the other, would take up, if it, endowed with a certain amount of *rigidity*, were traversed by a similarly directed current, similarly placed with reference to the magnet.

If, now, the magnet acted upon a conductor composed of as *many* pieces arranged in line along the direction of the currents, which pieces were all fixed at the end turned towards the negative pole, or at least displaced with difficulty in a direction at right angles to the direction of the current, but free at the other end, such a system, by breaking up into as many separate magnetic curves as there are separate currents, would show precisely the phenomena *which the stratified discharge presents to the magnet.* The phenomena, on the contrary, would be impossible if all the layers together formed only a single current between the kathode and anode.

The independence of the different portions of the discharge, for example, of the kathode-light with the first layer of the positive light is seen from the following. When the kathode-rays unroll themselves spirally, the first layer of the positive light does not follow the end of the negative rays in its revolutions, but the layer remains on the outside of the whole spiral on the side turned towards the anode, without having any connexion with the end of the ray on the interior of the spiral. Each layer behaves in the same way towards the preceding layer of positive light on the negative side.

In conclusion, it is easy to see how the view above explained removes the difficulties which arise out of the view till now usually held. From the kathode, as from a number of points lying between the two electrodes, which correspond to the limits of the positive layer towards the kathode, issue

a number of open currents, which render the gas incandescent in their path, and reach so much the further the greater the exhaustion is. If, when the exhaustion is not very great, the length of the discharge issuing from the kathode is less than the interval between the kathode and the next point of discharge (from which the first positive layer issues), there then must exist between the kathode-light and the first positive layer a space traversed by no current, in which therefore there is no luminosity, the so-called "dark space."

If the current-length of the kathode-discharge increases in consequence of increased exhaustion, so that it is equal to the interval between the kathode and the next point of discharge, then the kathode-rays reach the positive light and *the dark space vanishes.* If the length of the kathode-current become still greater than that interval, then the kathode-light advances into the space into which also flows a current from the second point of discharge, and the *kathode-light penetrates into the positive light.* In exactly the same way is explained the production of the dark space between each bundle of secondary negative light and the layer which follows it, and the dark spaces which the layers show between them at comparatively small exhaustion, whilst they are in contact when the exhaustion is greater.

In the same way the phenomena not to be explained by previous views, those obtained (pp. 178–183) with kathodes of different forms and positions, contain nothing more that is puzzling; and there is no more need to suppose a to-and-fro motion of electricity, or of a repeated zigzag path, nor of a new and non-luminous mode of discharge.

[To be continued.]

XXVIII. *Evolution by Subtraction.* *By the* Rev. F. H. Hummel, *M.A.**

"WHEN we require to find a root (say, *e. g.*, the square root) of an arithmetical quantity, we begin by 'pointing,' *i. e.* dividing off the digits of our given quantity into equal groups, starting from the units' place, each group containing a number of digits equal to the index of the required root; the last group on the left containing so many digits as happen to remain to it. On this last group we commence operations, finding the square root of the square number next below it in numerical value. The remaining steps are simply successive solutions of a continually recurring equation of the form $n^2=a^2+2ax+x^2$; in which, by approximating to x at each step, and then adding its approximate value to a for the next

* Communicated by the Author.

equation, we arrive at a value, actual or approximate, for n, the required root. For the cube root, the corresponding equation is considerably more involved; and beyond this point our authors abandon the method.

"Let us examine this traditional method of evolving square and cube roots. The second and following steps are methodical enough, being in fact solutions of a known equation, and lead surely to correct results. But the first step depends entirely on an exercise of memory; it is not performed by method, but by an arbitrary process of recalling previous knowledge; it is, in a word, guesswork. Take, for instance, the square root of 6561; having given that the first figure of the result is 8, the rest follows of course; but how do we know the first figure? Or suppose the given square were 64 simply; our text-books furnish absolutely no method whatever for finding the square root of a number of less than three digits.

"Yet there is a method, not mentioned in our books, a method of singular, almost ridiculous, simplicity, that will evolve with certainty the square root of any exact square whatever. The rule is no more than this :—*Subtract successively the even numbers; the last remainder will be the square root.* Here, for, example, is the square root of 64 above mentioned, evolved by an unerring rule. The process of subtraction has been continued until it left us the required root, and could not then be carried any further.

$$
\begin{array}{rr}
n^2 = & 64 \\
 & \underline{2} \\
 & 62 \\
 & \underline{4} \\
 & 58 \\
 & \underline{6} \\
 & 52 \\
 & \underline{8} \\
 & 44 \\
 & \underline{10} \\
 & 34 \\
 & \underline{12} \\
 & 22 \\
 & \underline{14} \\
n = & \underline{8}
\end{array}
$$

"Now let us turn to the cube root, and take this simple rule:—*For the* n*th subtrahend, multiply* n *by* 6, *and add the previous subtrahend. The last remainder will be the cube root.* Thus the first subtrahend is 6; the next, $6 \times 2 + 6 = 18$; the third, $18 + 18 = 36$; then $24 + 36 = 60$; and so on. The series

of subtrahends is, in fact, 6 × the series of 'triangular' numbers, 1, 3, 6, 10, &c. Here is the cube root of 729 found by this process.

$$\begin{array}{rr} n^3 = & 729 \\ & \underline{6} \\ & 723 \\ & \underline{18} \\ & 705 \\ & \underline{36} \\ & 669 \\ & \underline{60} \\ & 609 \\ & \underline{90} \\ & 519 \\ & \underline{126} \\ & 393 \\ & \underline{168} \\ & 225 \\ & \underline{216} \\ n = & \underline{9} \end{array}$$

"It is clear that similar rules may be framed for finding the roots of higher powers."

Considering this method of evolution to be of great importance, and believing it (under correction) to be wholly novel, I must at once assign the credit of its invention to the real author. To the foregoing portion of this paper I have contributed nothing but the wording; the matter of it was communicated to me by my esteemed friend and neighbour the Rev. W. B. Cole, of Shanklin. In the belief that the method is altogether new to the world (though its author seems to have retained it *in petto* for some years), I have ventured to give it the title of "Cole's Method of Evolution;" and with his permission I now present it to the public, with a few remarks of my own on its more extended application.

The first point to which I shall refer is the principle on which the method is based, so that we may obtain a general formula whence to frame rules of evolution for the fourth, fifth, or any higher roots. We shall then be in a position to decide on the extent of its application, and to frame tables for its employment.

Having given a known power of an unknown number (call it n^m), we subtract successively the terms of a series, and find for a last remainder the required root n. Obviously this series is the one whose sum to n terms $=n^m-n$; and obviously,

again, this series is the one whose general term

$$u_n = (n^m - n) - \{(n-1)^m - (n-1)\}.$$

If we substitute 1, 2, 3, &c. for n in this formula, we shall obtain the successive subtrahends for any given index m.

No confusion need be apprehended from the double sense in which I am using the symbol n, to indicate both the required root and the ordinal number of each term in the series; for during the operation the value of the root is unknown to us, and we substitute the integers successively in the hope of finding it; and a consideration of the series will show that the number of terms required is in every case $=n$, including the zero term for $n=1$.

The general term as given above is not yet in a form adapted for simple arithmetical calculation, except for very low values both of m and n; but by expansion we may cast it into a form capable of resolution into factors in such a way as to make the calculation easier. For

$$(n^m - n) - \{(n-1)^m - (n-1)\} = n^m - 1 - (n-1)^m;$$

which, expanded by the Binomial Theorem, the highest powers of n cancelling, leaves us

$$u_n = -1 + mn^{m-1} - \tfrac{1}{2}m(m-1)n^{m-2} + \&c. \pm mn \mp 1.$$

Here, if m be even, the last term is -1, and the expression for

$$u_n = 2\Big\{-1 + \tfrac{1}{2}mn\Big(n^{m-2} - \tfrac{1}{2}(m-1)n^{m-3} + \&c. - \tfrac{1}{2}(m-1)n + 1\Big)\Big\};$$

if m be odd, the last term is $+1$, which cancels, and the expression becomes

$$u_n = mn\Big(n^{m-2} - \tfrac{1}{2}(m-1)n^{m-3} + \&c. + \tfrac{1}{2}(m-1)n - 1\Big).$$

In this latter case the quantity within the bracket will always be divisible by $(n-1)$, as may be seen by an inspection of the coefficients. So far, then, the method appears to favour the odd indices, which might have been expected to present the greatest difficulty; and an even index, if not a power of 2, may be reduced to an odd one by one or more operations for the square root.

But an alternative rule offers itself, of making each term of the subtrahend series to be itself the sum of a series. Such a series will of course be that of the differences of successive subtrahends; and in working we shall have to add to each term the last preceding subtrahend, which will have been the sum of all the previous terms. The general term of this series will be $u_{n+1} - u_n$. Now this expression

$$= (n+1)^m - 1 - n^m - \{n^m - 1 - (n-1)^m\}$$
$$= (n+1)^m + (n-1)^m - 2n^m.$$

The first term of this addition is cancelled by the $-2n^m$; the expression will accordingly be the double of the positive terms of the expansion of $(n-1)^m$, beginning at the third term. If m is even, the last term will be $+2$. Therefore we may write as an alternative formula,

$$u_{n+1}-u_n=m(m-1)n^{m-2}+\frac{\lfloor m}{\lfloor m-4}\cdot\frac{n^{m-4}}{3\,.\,4}+\frac{\lfloor m}{\lfloor m-6}\cdot\frac{n^{m-6}}{3\,.\,4\,.\,5\,.\,6}$$
$$+\,\&\text{c.}\ [+2 \text{ if } m \text{ is even}].$$

All these coefficients are integral. When m is even, they will recur in reverse order, like those of the Binomial Theorem, and the whole expression will be divisible by n^2, with the final 2 for remainder. When m is odd, the whole expression will be divisible by n; and when m is prime, it will also be divisible by m. When $m-1$ is prime, it will be divisible by $(m-1)n^2$ with remainder 2. When m is even but not a multiple of 4, it will be divisible also by (n^2+1) with remainder 2.

We have thus for any integral value of m, whether odd or even, two alternative formulæ, in either of which, by substituting 1, 2, 3, &c. successively for n, we may obtain the terms of the required series, either directly, or by summing an ancillary series; and by continuously subtracting these terms in their order from the given quantity n^m, we should at last find n as a last remainder, at a point beyond which no further subtraction is possible, since every term of the series is greater than n. As both these formulæ depend upon the value of m, it will be possible to establish by means of them an arithmetical rule of evolution for any positive integral index whatever; it becomes simply a question of ingenuity in each case to cast one or other of the formulæ into the most convenient shape for working.

The rule already given for the square root may be derived directly from the recognition of $n^2-n=n(n-1)$ as double the sum to n terms of the series of consecutive integers; but by our formulæ $u_{n+1}=2n$; or $u_{n+1}-u_n=2$, the difference of successive even numbers. So for the cube root, $u_{n+1}-u_n=6n$, or $u_{n+1}=3n(n+1)$—formulæ which correspond to the two forms of the rule as stated above. In like manner we may find a rule for extracting the 4th root thus: for the $(n+1)$th subtrahend, multiply n^2 by 12, and add $2+$ the previous subtrahend; for if $m=4$, $u_{n+1}-u_n=12n^2+2$.

Now we are well outside the pale of the old rules, which stopped short at the cube root. I propose to venture a few steps into the unexplored region, and find series and rules for some of the higher indices by means of the formulæ already established. Taking the integers in order for values of m, and working out their series, we may discover from the lower

values some further properties which may reduce the labour of calculation for the higher values.

Let us begin by working out a sum in the 5th root.

Ex. Find the 5th root of 1048576.

Here $m=5$.

$\therefore\ u_{n+1}-u_n=20n^3+10n=10n(2n^2+1)=10n\{n^2+(n^2+1)\}$.

Hence the Rule:—Cut off the final digit of the given number, and commence the subtraction at the tens' digit. For the $(n+1)$th subtrahend, add to n^2 the next higher number, multiply the sum by n, and add to the product the previous subtrahend. Replace the final digit to the last remainder.

$1^2=1$	$2^2=4$	$3^2=9$	$4^2=16$
+2	+5	10	17
3	9	19	33
	×2	3	4
	18	57	132
	+3	21	78
	21	78	210
$5^2=25$	$6^2=36$	$7^2=49$	$8^2=64$
26	37	50	65
51	73	99	129
5	6	7	8
255	438	693	1032
210	465	903	1596
465	903	1596	2628
$9^2=81$	$10^2=100$	$11^2=121$	$12^2=144$
82	101	122	145
163	201	243	289
9	10	11	12
1467	2010	2673	3468
2628	4095	6105	8778
4095	6105	8778	12246
$13^2=169$	$14^2=196$	$15^2=225$	
170	197	226	
339	393	451	
13	14	15	
4407	5502	6765	
12246	16653	22155	
16653	22155	28920	

$n^5=$ **104857(6**
3
104854
21
104833
78
104755
210
104545
465
104080
903
103177
1596
101581
2628
98953
4095
94858
6105
88753
8778
79975
12246
67729
16653
51076
22155
28921
28920
$n=$ **16**

[Incidentally we may note that this rule for the 5th root affords an easy proof of the arithmetical proposition that the final digit of any number is the same as that of its 5th power, and consequently of any $(4p+1)$th power, p being any positive integer.]

I have given this example worked out, with the marginal calculations, to show what such a sum in this method will look like, and to demonstrate the facility of the working. I shall content myself with subjoining rules for the $(n+1)$th subtrahend for each of the next half-dozen series.

6th root. $u_{n+1}-u_n=30n^2(n^2+1)+2$. Rule:—multiply n^2 by the next higher number, and again by 30, and add 2+ the previous subtrahend.

7th root. $u_{n+1}=7n(n+1)\{n(n+1)+1\}^2$. Take the product of n and $(n+1)$, multiply it by the square of the next higher number, and again by 7. But for the higher values of n the trouble of squaring will lead us to prefer the alternative formula, $u_{n+1}-u_n=7n\{6n^2(n^2+1)+4n^2+2\}$; multiply n^2 by the next higher number, and again by 6; add to the product 4 times n^2 and 2; multiply the sum by n, and again by 7, and add the previous subtrahend.

8th root. $u_{n+1}-u_n=28n^2(n^2+2)\{n^2+(n^2+1)\}+2$. Add to n^2 the next higher number, multiply the sum by (n^2+2), and again by n^2, and again by 28, and add 2 + the previous subtrahend.

9th root. $u_{n+1}=3n(n+1)\{3(n^2+n+1)^3+n^2(n+1)^2\}$; a formula which will be more convenient for higher terms in the shape

$$3n(n+1)\Big[n(n+1)\{3[\{n(n+1)+1\}\{n(n+1)+2\}+1] \\ +n(n+1)\}+3\Big].$$

Find the product of $n(n+1)$; take the product of the next two consecutive numbers above it, add 1, multiply by 3, add $n(n+1)$; multiply the sum by $n(n+1)$, add 3, and multiply by $3n(n+1)$.

10th root.

$$u_{n+1}-u_n=30n^2(n^2+1)\{3n^2(n^2+3)+n^2+(n^2+3)\}+2.$$

Take the product of n^2 and (n^2+3), multiply by 3, and add to the product n^2 and (n^2+3); multiply the sum by n^2, again by (n^2+1), and again by 30, and add 2 + the previous subtrahend.

11th root.

$$u_{n+1}=11n(n+1)\{n(n+1)+1\}[n(n+1)\{n(n+1)+1\} \\ \{n(n+1)+3\}+1].$$

Take the product of $n(n+1)$, multiply it by the next higher

number, and again by the next odd number above that; add 1 to the product, and multiply by the product

$$n(n+1)\{n(n+1)+1\}$$

already found, and again by 11.

Let these examples suffice; we have reached a point at which the formulæ begin to have a complicated appearance, though the practical working of them is not beyond the capacity of a child, and already we have made a great advance on the results of the older methods. We may already perceive from our investigation that the formulæ for $u_{n+1}-u_n$ are of most use in simplifying results for the lower indices; but that as we go higher we can more safely rely on the formulæ for u_n which more closely follow the Binomial Theorem, and so, yielding readily to division by such factors as $(n+1)$, (n^2+n+1), &c., often give us combinations of consecutive or nearly consecutive integers, which are far more easily worked in practice than described in rules.

Now let us tabulate the results of the rules already found. I will give ten terms of each series, so that they may be easily verified by finding their sum in each case to be 10^m-10; that is, a row of nines with a zero at the end.

$m=1$. All the terms are zero.

$m=2$. $0+2+4+6+8+10+12+14+16+18+$ &c.

$m=3$. $0+6+18+36+60+90+126+168+216+270+$ &c.

$m=4$. $0+14+64+174+368+670+1104+1694+2464$
$+3438+$ &c.

$m=5$. $0+30+210+780+2100+4650+9030+15960$
$+26280+40950+$ &c.

$m=6$. $0+62+664+3366+11528+31030+70992+144494$
$+269296+468558+$ &c.

$m=7$. $0+126+2058+14196+61740+201810+543606$
$+1273608+2685816+5217030+$ &c.

$m=8$. $0+254+6304+58974+325088+1288990+4085184$
$+11012414+26269504+56953278+$ &c.

$m=9$. $0+510+19170+242460+1690980+8124570$
$+30275910+93864120+253202760+612579510+$ &c.

$m=10$. $0+1022+58024+989526+8717048+50700550$
$+222009072+791266574+2413042576$
$+6513215598+$ &c.

$m=11$. $0+2046+175098+4017156+44633820+313968930$
$+1614529686+6612607848+22791125016$
$+68618940390+$ &c.

Having now the actual numbers before us, we can verify in fact the relation which we may find from the algebraic formulæ to exist between the terms of consecutive series. Denoting the number of a series by a bracketed index, we may write this relation thus: $u_n^{(m+1)} = nu_n^{(m)} + \Sigma_{n-1}^{(m)} + 2(n-1)$; the nth term of any series $= n$ times the nth term of the preceding series + the sum of the $(n-1)$ previous terms of that series + twice $(n-1)$. Thus, having given five terms of the 6th series, the 5th term of the 7th series will be

$$5 \times 11528 + 62 + 664 + 3366 + 2 \times 4 = 61740.$$

By this formula we may extend our tables to any number of series, and to any number of terms in each.

We may notice in passing the recurrence of final digits of every five terms of each series, repeated in every group of four series, thus:

$$m=4p,\ \dot{0}, 4, 4, 4, \dot{8};\quad m=4p+1,\ \dot{0}, 0, 0, 0, \dot{0};$$

$$m=4p+2,\ \dot{0}, 2, 4, 6, \dot{8};\quad m=4p+3,\ \dot{0}, 6, 8, 6, \dot{0}.$$

So the sum of $5p$ or $5p+1$ terms in every series, and of $5p-1$ terms of the odd series, have zero for the final digit, as would be necessary for powers of numbers of the form $5p$ or $5p \pm 1$. All the terms of each series are even numbers, as we might have foreseen from the algebraic formulæ. This, too, was necessary; for the powers of odd numbers being all odd, and those of even numbers all even, $n^m - n$ is in every case even.

So much, then, for the mode of constructing the subtrahend series. Before proceeding further, I may remark that this method is of course as useful for involution as for evolution; for being in possession of the series whose sum to n terms is $n^m - n$, we have only to add that sum to n to obtain the value of n^m.

Hitherto we have seemed to assume that the number whose mth root is to be taken is the exact mth power of an integral root. But clearly, if this were an indispensable condition for the applicability of the method, its use would be extremely limited. Let us consider the case in which the given number is not an exact mth power; we may write it $n^m \pm d$. As the evolution is performed throughout by the process of subtraction, every remainder will differ by the same excess or defect $\pm d$ from what it would have been if the given number had been n^m exactly. Accordingly, our last remainder will be $n \pm d$; that is, we have left a number which differs from n by the same excess or defect as that by which the given number differed from n^m. As this is by far the most usual case, it is evident that the last remainder is not, after all, the right place

to look even for an approximation to the required root, as a general rule. But our investigation of the series has shown us that $n^m - n$ is always the sum of n terms; and consequently, in approximating to the mth root of $n^m \pm d$, we can discover n at once by counting the number of terms subtracted. It must be remembered that the first term is in every case zero, and therefore the number of actual subtractions we have performed will be $n-1$.

Having thus found n, which is so far an approximation to the required root, we may now obtain a nearer approximation by the help of the last remainder, which we have seen to be $n \pm d$. For we can now determine the value and sign of d by subtracting n from the last remainder, and we know that our given number is $n^m \pm d$. There are, of course, known methods by which the mth root of $n^m \pm d$ may be pretty accurately found; but, omitting these, let me point out that this method itself will furnish a means of approximating tolerably nearly to the root, by adding to n (or, if the sign of d prove to be negative, subtracting from n) the quotient of d divided by the next term of the subtrahend series following the last one subtracted. To write it algebraically,

$$\sqrt[m]{(n^m \pm d)} = n \pm \frac{d}{u_{n+1}} \text{ nearly.}$$

For example, let it be required to find the 11th root of 8590035271. After 7 subtractions we arrive at a last remainder 100687. Therefore $7+1=8$ is an approximate root. The next subtrahend would have been 22791125016; accordingly we may take as a still closer approximation to the required root

$$8 + \frac{100687-8}{22791125016} = 8{\cdot}000044 \text{ nearly.}$$

Our investigation, then, will lead us to a restatement of Cole's method in some such form as this:—

To find the mth root of a given number, find a sufficient number of terms of the series whose sum to n terms is $n^m - n$; subtract these terms successively from the given number as far as such subtraction is possible. The number of such subtractions $+1$, if it be equal to the last remainder, will be an exact mth root. If not, it will be an approximate mth root; and for a nearer approximation, find the difference between this first approximate root and the last remainder, and add (or if the remainder was the less, subtract) the quotient of that difference divided by that term of the series which follows next after the last term subtracted.

XXIX. *On the Reversal of the Developed Photographic Image.* *By* Captain ABNEY, *R.E., F.R.S.**

[Plates V. & VI.]

IN the Proceedings of the Royal Society † I explained the theory of the reversal of the photographic image on development, showing that it was due to oxidation when ordinarily met with. On looking back, however, to the Philosophical Transactions for 1840, I find Sir J. Herschel gave a description of a similar phenomenon which perhaps might admit of a different explanation. This subject I examined some time ago; and since the whole question seems to have revived in interest, it has seemed an opportune moment to put on record my more recent investigations. I need scarcely say that the fact as to the reversal of the image is any thing but new, as will be seen from Herschel's, Hunt's, and Draper's well-known works. The explanation, however, as far as I know, has been confined to my researches already mentioned ; and these were summarized very briefly.

Sir John Herschel, in his memoir just referred to, experimented on a prepared paper. In article 48 of his communication he says, "A paper endowed with a pretty high degree of sensibility may also be prepared with the following triple solution, viz.:—1st, acetate of lead; 2nd, hydriodate of potash; 3rd, nitrate of silver . . . . If paper so prepared and darkened in the sun be washed over with a fresh dose of hydriodate, the exposure to sunshine being sustained, it whitens with great rapidity ; and were it practicable (which I have not found it) to ensure precisely the same ingredient-proportions, and the same degree of blackening in the sun to start from, I should not hesitate to propose this as an excellent process for a positive photographic paper."

This process in Sir J. Herschel's hand was not then uniformly successful; and it must be noted that here we are dealing with a visible image, and not a developable one. But it will be found that the same argument applies to both, since the visible image and the developable image are of precisely the same nature, varying only in the matter of degree.

It has been some time known, and more recently has been brought foward by Dr. Angus Smith‡, that a slightly acidified solution of potassium iodide liberates iodine in the pre-

* Communicated by the Author.

† Vol. xxvii. pp. 291 & 451.

‡ Since writing this paper, the author has noted a paper in the Philosophical Magazine for August 1880, by Dr. Leeds, in which this solution has been investigated.

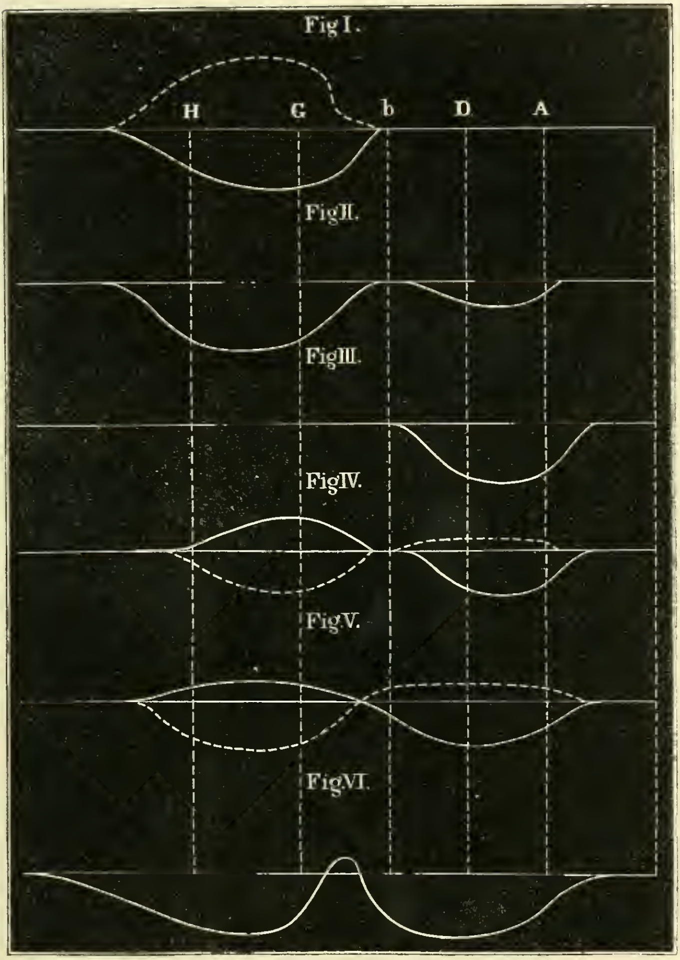


Plate showing reversing actions of Iodides, Bromides, and oxidizing agents on AgI.

N.B.—The curves below the line show the reversals or positive images; the curves above the line show the ordinary action, or negative images; the ordinates approximately represent the amount of action.

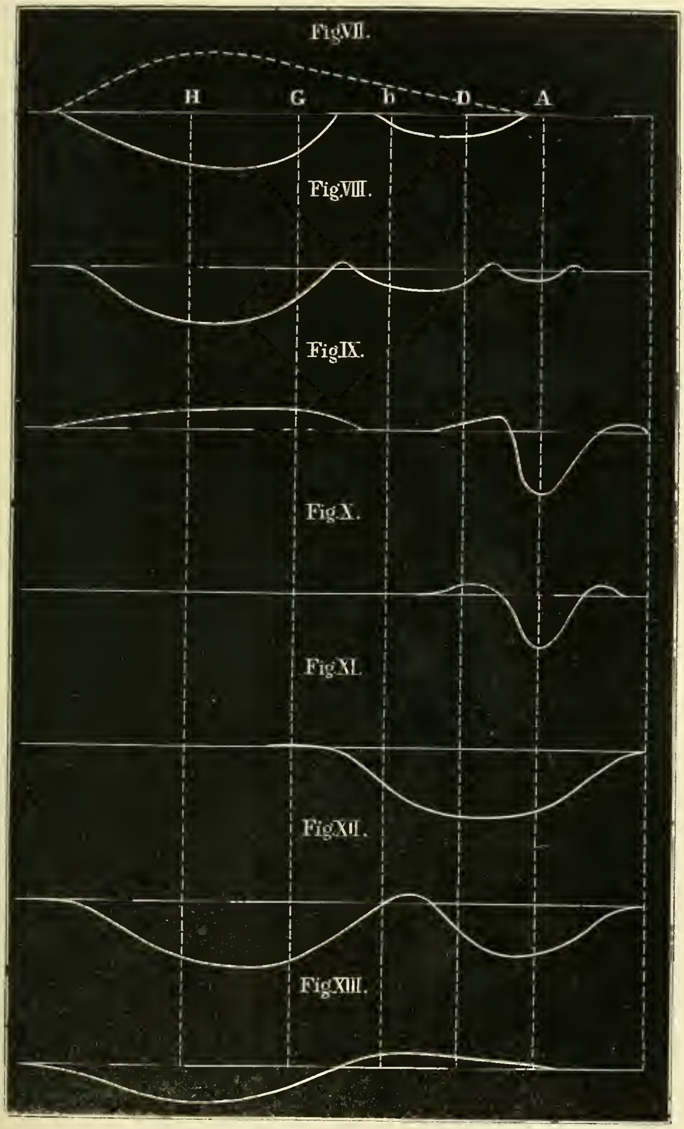


Plate showing reversing action of Bromides and oxidizing agents on AgBr.

N.B.—The curves below the line show the reversals or positive images; the curves above the line show the ordinary action, or negative images; the ordinates approximately represent the amount of action.

sence of light, though it remains unaltered in the dark for considerably long periods. If, then, paper impregnated with a silver-salt be blackened by light and be then treated with potassium iodide, we have the exact explanation of Sir John Herschel's experiment, presuming the paper be slightly acid. When iodide of silver is exposed to light in the presence of a neutral solution of silver nitrate, we have this acidity produced, the iodine liberated from the silver iodide in its conversion to subiodide combining with the silver nitrate and liberating nitric acid, probably with the formation of an iodate. This is true more especially when the paper is not absolutely desiccated; for when desiccation is perfect the iodine might not combine. Hence may arise the uncertainty to which Sir J. Herschel refers. If paper so prepared be kept damp by any means, the reaction will invariably take place, and iodine from the potassium iodide will be liberated and combine with the semi-metallic silver on the paper to produce silver iodide; in other words, the blackened surface will bleach.

The experiment may be tried in a variety of ways. The simplest, perhaps, is to salt ordinary unglazed paper with a 10-per-cent. solution of common salt, and when dry to float it on a solution of silver nitrate of about the same strength, and then to dry and expose it to the daylight to blacken. When the blackening is produced, if the paper be slightly washed and then be treated with a 5-per-cent. solution of potassium iodide (slightly acidified with nitric acid) in the dark, and while still damp be exposed *beneath a negative* to the light, it will be found that those portions beneath the transparent portions will rapidly bleach, and we shall have a *negative image* instead of a positive, but reversed as regards right and left.

The same experiment may be repeated, substituting potassium bromide for the iodide, and the same results will be obtained. It may be asked if any metallic iodide or bromide will be effective; and to this an affirmative answer may be given; but the use of acid is not necessary in all cases. Those metals which form two iodides or bromides must be used extremely dilute, or the bleaching will take place in the dark—that is, supposing the highest type of bromide or iodide be employed. Thus a strong solution of zinc iodide may be used and acidified, whilst a very dilute solution of ferric iodide must be used.

This, then, is the explanation of the reversal of the visible image; and it now remains to show that the same action takes place in the invisible image.

It is well known, if a plate be prepared with silver iodide by the ordinary wet process, be briefly exposed to light, and

after washing be treated with a solution of potassium iodide and then be exposed to an image in the camera, that, after dipping in the silver-bath and developing, a positive image is obtained. It matters not whether the potassium iodide be alkaline, neutral, or acid, the same effect will be noted; also that there is no difference if, after treatment with the potassium iodide, the plate be washed or not, the reversal of the image will still be shown. In this case the iodine is liberated as before, but the action is increased by the access of oxygen from the air; in fact it is a mixture of effects.

If potassium bromide or any simple bromide be substituted for the iodide, the same result obtains. Silver iodide, if prepared with an excess of soluble iodide, or if, after preparation with excess of silver, it be treated with a soluble bromide, is insensitive to light; and the explanation of this perhaps may be found in the fact already stated.

It has been usually held that a soluble iodide, such as potassium, can destroy an invisible impression made by radiation; but this is not the case if it be treated with the iodide in the dark. If, however, any iodide, such as cupric or ferric, be employed, which readily liberates an equivalent of iodine, the destruction is accomplished in the dark. The least favourable iodides for such destruction, as I have already shown*, are the monads.

If a plate prepared with silver iodide have a preliminary exposure given it, and then be exposed *for a considerable time* to the image formed in the camera, a reversal of the image will take place as before. If, however, such a plate, after washing, be treated with an aqueous solution of pyrogallic acid, potassium nitrite, or any other deoxidizing agent, such reversal of the image will not be obtained; nor will it if it be exposed in a cell containing such a substance as benzene, or if exposed in dry hydrogen. From this we learn that, to obtain reversal, oxygen must be present in some form or another, and that, if a substance readily taking up oxygen be in contact with the silver-salt, a reversal cannot be readily obtained.

An interesting corroboration of the above statement is to be found in the treatment of an exposed plate in a cell containing a dilute solution of permanganate of potash, bichromate of potash, or hydroxyl, when it will be found that the reversal takes place with the greatest facility. The same reversals may also be obtained by using any of the mineral acids in a diluted form†.

* Photographic Journal, 1878.

† It must, however, be remembered that the solutions must be very dilute, or the whole effect of the preliminary exposure will be destroyed, since these oxidizing agents are active in the dark, but act more readily in the light.

The above experiments show, then, that a reversal may be obtained by the presence of the iodides or bromides (and in a more feeble manner, I have also found, by that of the chlorides), and also by oxidizing agents and mineral acids; whilst the presence of a deoxidizing agent, or the exposure of the plate in a medium free from oxygen, prevents the occurrence of the phenomenon.

We shall consider shortly as to whether the reversing action depends upon the sensitiveness of the salt of silver obtained by the preliminary exposure, or upon that of the agents employed in effecting such reversal.

With the bromide of silver we have rather different phases of the phenomenon to consider. The development can be carried out with the alkaline or the ferrous oxalate developer, a mode which is more easy to carry out than the development by precipitation of metallic silver from an aqueous solution of silver nitrate. For experimental purposes, films containing silver bromide may be formed of collodion or of gelatine; and the behaviour of the silver-salt in the two vehicles is somewhat different, and has to be considered separately. Collodion is, or should be, a strictly neutral substance; that is, it is merely a medium in the pores of which the silver-salt is entangled and kept in position, and has no effect on the progress of development or on the action of light, beyond that which may be due to its physical qualities, its chemical constitution remaining unchanged.

A collodion film is essentially porous and not continuous, as may be seen by a microscopic examination; and free access of the atmosphere to the silver is thus obtained. Gelatine, on the other hand, is a substance readily acted upon by oxidizing agents and by the halogens; and consequently it may have an effect on the progress of development and on the action of light, its chemical constitution becoming altered. It is a homogeneous film, and not porous in the ordinary sense of the word, and is a protective agency against the atmosphere to those silver-salts which may be embedded in it.

The most convenient method of experimenting with silver bromide is in the form of emulsion, made either with collodion or with gelatine; but it is not to the purpose of the present paper to refer to the mode of preparation beyond stating that in the former case the emulsion is usually prepared with an excess of silver nitrate, and the latter with an excess of soluble bromide, both of which are eliminated as far as possible by washing.

If a film containing silver bromide, whether in gelatine or collodion, have a preliminary exposure given to it, and then

be treated with a soluble bromide of an alkali, such as of potassium, and be again exposed to light in the camera, it will be found that there is not such a rapid reversal of the image as with the iodide, but that longer exposure is required to effect it, the reason being that bromide of silver prepared with a large excess of soluble bromide is still sensitive to light. If, therefore, the light decomposes the soluble bromide on the plate, liberating enough bromine to form fresh bromide of silver with the subbromide formed by the preliminary exposure, that freshly formed bromide, being sensitive to light, is again reduced to the subbromide state by the same rays which formed it. It will be evident, however, that reversal should take place more rapidly with the soluble bromide present than without it; and such is the case.

It is useless to treat a silver bromide film with a soluble iodide, since silver iodide is immediately formed, and the reactions that take place are similar to those already described.

If bromide of silver in collodion be exposed to the image in the camera without the presence of any other substance, a reversal takes place. Roughly speaking, the reversal takes some sixty times more exposure to the light than is requisite to produce the maximum ordinary effect. To trace the cause of this reversal it is only necessary to treat the film with a 5-per-cent. solution of potassium nitrite, when it will be found that the reversal does not take place. The same holds true when the film is treated with any deoxidizing solution, or if the plate be immersed in benzene or hydrogen. The cause, then, of the reversal in this case is evidently an oxidation; and this may be further verified by treating the film, after a preliminary exposure, with bichromate of potash, hydroxyl, &c.; it will then be found that the reversal takes place much more rapidly than when these oxidizing agents were absent. The same may be said of the mineral acids.

If silver bromide be held in a gelatine film, the action of light is somewhat different. If the plate be exposed in the camera for a short time, say a few seconds, the image develops in the usual manner and we have a negative image; if it be prolonged to, say, a minute, the image is reversed on development; a further exposure causes a negative image to be produced, whilst one much more prolonged causes a positive image again to be formed on development. Here are four distinct phenomena* which need explanation. To solve the problem offered, plates should be exposed when saturated with a solution of potassium nitrite as before, when it will be found

* Mr. C. Bennett described these phenomena in the 'British Journal of Photography' in 1878.

that the phenomena are absent, a reversal being almost impossible to obtain unless the length of exposure be such as to thoroughly oxidize the nitrite at the expense of gelatine. For ordinary purposes it may be said that a reversal is non-existent under these conditions.

If a plate be exposed in benzene, however (a liquid which does not permeate through gelatine), the phenomena are still existent. If a plate be exposed to such an extent that there is a marked image apparent before development, and be then immersed in water, it will be found that when the image appears the gelatine refuses to swell to the same extent that it does when the light has not acted. Taking these two experiments together, it is evident that the gelatine has played some part with the silver bromide. It may therefore be presumed that the three last phenomena are due, the 1st to the oxidation of the surface-particles of the bromide and a consequent change in colour, the 2nd to the change in colour of these particles permitting the coloured rays to which it is sensitive to strike a deeper layer, and the 3rd to the oxidation of this layer at the expense of the gelatine. The 3rd and 4th phenomena are so unimportant that they are scarcely worth investigating. The presence of organic matter is evidently necessary for their appearance; at least I have never been able to obtain them with collodion films not containing a preservative.

As before, the experiment of saturating one of these gelatine films with bicromate of potash shows that the reversing action is very much increased by the presence of the oxidizing agent. Mr. Bolas* has recently described a plan of producing reversed negatives by allowing the bichromate to dry in the film, which is a practical application of this reversing action of light in the presence of an oxidizing agent.

A convenient method of showing these phenomena on the same plate is to use a screen containing squares of graduated opacity, as suggested by the editor of the 'Photographic News,' or such as the sensitometer prepared by Mr. Warnerke, and procurable at most photographic warehouses.

Having treated of these reversals of the image in a general way, it now remains to show which radiations are effective in producing them. For testing this, spectro-photography was resorted to, a special dark slide having been constructed capable of holding a cell which would contain the plate, and be immersed in a liquid or any gas or vapour whose action it might be desired to test. Three flint-glass prisms were used, and a lens to the camera of about 2 feet equivalent focus, the

* Photographic Journal, May 1880.

collimating lens being a duplicate of it. The time of exposure was, as a rule, three minutes to the sunlight or to that of the electric arc, care being taken in the latter case that an image of the positive pole fell on the slit so as to give a continuous spectrum. The action of potassium iodide on silver iodide will first be described.

A plate was exposed after being sensitized, and after washing was immersed in a cell containing a 1-per-cent. solution of potassium iodide and exposed to the spectrum. The result is shown in Plate V. fig. 1; the same rays which cause an image to be formed in the usual manner likewise caused a reversal (dotted curve, fig. 1).

A plate similarly prepared was exposed in a 1-per-cent. solution of potassium bromide for the same length of time, with the result that a reversal was obtained in the blue and likewise in the red, but much less marked in the latter (fig. 2). These two experiments tend to prove that, in reality, it is the bromide that is acted upon to some extent, and the effect is not entirely due to the silver-salt. This was particularly manifest in the case of the iodide and bromide slightly acidified with a mineral acid, and was much less marked when the solution was alkaline—in the latter case, the reversal taking place in the blue, and not in the red regions of the spectrum.

To see if the silver-salt had any marked effect on the rapidity of oxidation, a silver-iodide plate was washed, given the same preliminary exposure, and then placed in the spectro-photograpic apparatus without any surrounding fluid. A reversal was obtained in the blue, but not to any thing like such an extent as when placed in soluble iodides or bromides. The reversal, therefore, when the plate is exposed in the latter is partially due to the action of radiation on the bromide, and partly to that exerted on the silver-salt itself.

A silver-iodide plate, treated as before, was next exposed in a weak solution of potassium bichromate, when there was a strong reversal in the red (fig. 3), and no action whatever in the blue. Permanganate of potash was next substituted for the bichromate; and the same reversing action was found, with the addition of a negative image in the blue (fig. 4).

With hydroxyl the same phenomena were observed as with the permanganate, the reversal taking place a little further into the green (fig. 5). Studying the absorption due to these three oxidizing agents, it would appear that the reversing action *is due to the action of light on the salt of silver, which is changed by the preliminary exposure to light, and not to the action of light on the medium in which the plates are placed.*

With mineral acids a reversal was always obtained in the

red and in the blue, a portion of the spectrum in the green and yellow remaining unreversed (fig. 6). Now the action of these acids is not a strictly oxidizing action, but is probably a removal of the loose atoms of the silver which goes to form the subiodide, and leaving silver iodide behind as the result of the action. The results of the action of acids do not, therefore, vitiate the above deduction. A plate exposed in benzene or in nitrite of potash showed no reversal even with a very prolonged exposure. It should be remarked that the action of permanganate and bichromate of potash when very feeble is sometimes to give feeble negative images in the red and blue in lieu of positive images; also positive images in the blue, and feeble negative images in the red (see dotted curves in figs. 4 and 5). But this is to be accounted for by the fact that the dilution of these oxidizing agents is so extreme that the reducing action on the unaltered sensitive salt is far greater than the rapidity of the oxidation*.

Ordinary bromide of silver in collodion or gelatine may be taken as giving almost identical results under the influence of a soluble bromide †. Now ordinary bromide is sensitive as far as B (see dotted curve, Pl. VI. fig. 7); and it might be presumed that this sensitiveness to the rays of lower refrangibility would cause a modification in the action of the soluble bromide. A reference to figures 7 and 8 will show that this is the case, but that at the same time the features which are so marked with the action of bromide on silver iodide are present. Fig. 7 is the curves due to silver bromide in collodion which had received a preliminary exposure and was then exposed in a 5-per-cent. solution of acid potassium bromide. It will be seen that the curves in figures 7 and 2 are similar, showing that the principal action is due to light acting on the soluble bromide in the presence of an acid. Fig. 8 is a similar plate exposed in an alkaline solution of KBr, in which there is a modification of the curve. The last loop is probably due to the silver subbromide itself, since the oxidation of this salt by oxidizing agents occupies approximately the same position (see fig. 10).

Fig. 9 shows the effect of permanganate of potash; and when it is compared with fig. 10, which is the curve due to oxidation by bichromate of potash, it will be manifest that the

* There is one singular fact to be noted in this, and which I propose to treat of in another contribution, viz. that the iodide of silver, when given a preliminary exposure, is sensitive in a region of the spectrum lying between a point near D and one near A. This phenomenon has been described before and not explained, though experiments show that the explanation is easy.

† As before explained, it is useless to expose such plates in a solution of soluble iodide, since silver iodide is immediately formed.

chief oxidizing action lies in the red and ultra red of the spectrum.

Fig. 11 also shows the effect of bichromate of potash on silver bromide given a preliminary exposure, the plate in this case being a gelatine plate. It will be seen that the bichromate totally arrests all action in the blue, whilst it rapidly causes a reversal in the red.

Fig. 12 shows the effect of mineral acids on silver bromide, by which it will be seen that a maximum of reversal takes place in the red and in the blue. As before stated in regard to the iodide, the action of these acids can scarcely be regarded as an action of oxidation.

Fig. 13 shows the phenomena due to overexposure of silver bromide, by which it will be seen that reversal takes place in the blue and not in the red. Comparing this with figs. 7, 11, and 12, the effect of extraneous matter in causing a reversal is very marked.

Collodion plates exposed in benzene, or in aqueous solutions of pyrogallic acid, potassium nitrite, and sodium sulphite gave no reversal whatever.

Gelatine plates exposed in benzene gave the phenomena shown in fig. 13, whilst with the other media no reversal at all was obtained.

The explanation of the apparent contradiction shown by the behaviour of a gelatine plate exposed in benzene has already been given.

The actions of many other liquids and gases* have likewise been tried; but it was thought that the examples given sufficed, since they all pointed to the same conclusions, which may be summarized as follows:—

1st. The reversal of an image is due, in the majority of cases, to the oxidation of the subsalt of silver which formed by the first impact of light on the exposed salt of silver.

2nd. The oxidation is due to the action of light, the rays of lower refrangibility being the most powerful accelerators of oxidation.

3rd. Reversal of an image may be due to the presence of any haloid of an alkali, the reversal in this case being partly due to the action of light on such a haloid, and partly due to the tendency to oxidation of the subsalt of silver.

4th. The presence of a mineral acid tends powerfully to cause a reversal.

* Ozone was most marked in its oxidizing properties, and gave a curve very similar to fig. 12, both with the iodide and bromide of silver.

XXX. *Intelligence and Miscellaneous Articles.*

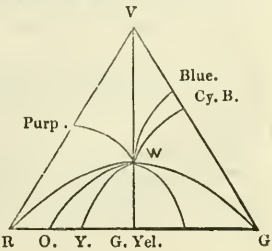
ON THE EFFECTS PRODUCED BY MIXING WHITE WITH COLOURED LIGHT. BY PROF. O. N. ROOD, OF COLUMBIA COLLEGE.

IT was noticed several years ago that when white light was mixed by the method of rotating disks with light of an ultramarine (artificial) hue, the result was not what one would naturally have expected; viz. instead of obtaining a lighter or paler tint of violet-blue, the colour inclined decidedly toward violet, passing, when much white was added, into a pale violet hue. Two attempts have been made to account for this curious fact. Brücke supposes that the light which we call white is really to a considerable extent red, and that the mixture of this reddish-white light with the blue causes it to change to violet. Aubert, on the other hand, following a suggestion of Helmholtz, reaches the conclusion that violet is really only a lighter shade of ultramarine-blue. He starts with the assumption that we obtain our idea of blue mixed with white from the sky, which, according to him, is of a greenish-blue colour. We then apply, as he thinks, this idea to the case of a blue which is not greenish, namely to ultramarine-blue, and are surprised to find that the result is different.

It will be shown in the present paper that these explanations are hardly correct, since they fail to account for the changes which, according to my experiments, are produced in other colours by an admixture of white. I prepared a set of brilliantly coloured circular disks which represented all the principal colours of the spectrum and also purple; these disks were then successively combined in various proportions with a white disk, and the effects of rapid rotation noted, a smaller duplicate coloured disk uncombined with white being used for comparison. Under these circumstances it was found that the addition of white produced the changes indicated in the following table:—

Vermilion became somewhat purplish.
Orange became more red.
Yellow became more orange.
Greenish yellow was unchanged.
Yellowish green became more green.
Green became more blue-green.
Cyan-blue became less greenish, more bluish.
Cobalt-blue became more of a violet blue.
Ultramarine (artificial) became more violet.
Purple became less red, more violet,

Exactly the same effects can be produced by mixing violet with the above-mentioned colours. Let R, G, V represent the three angles of Maxwell's colour-triangle, W being the position of white. Now, according to the received theory, as we mix white with different colours we advance in straight lines from the angles or sides of the triangle toward W; in point of fact, however, I find, as a result of the above-mentioned experiments, that we advance in curves toward W, these curves being similar to those roughly indicated in the figure. The only advance in straight lines is along the line joining violet with its complement greenish yellow. The other lines



are disposed symmetrically about this line as an axis. These experiments serve to explain the singular circumstance, that when complementary colours are produced by the aid of polarized light, it is difficult or impossible to obtain a red which is entirely free from a purplish hue, a quantity of white light being always necessarily mingled with the coloured light. In the case of the red, orange, yellow, ultramarine, and purple disks, I succeeded in measuring the amount of violet light which different proportions of the white disk virtually added to the mixture, and found that it was not directly proportional to the amount of white light added, but increased in a slower ratio, which at present has not been accurately determined.

For the explanation of the above-mentioned phenomena, Brücke's suggestion that white light contains a certain amount of unneutralized red light is evidently inapplicable, since the effects are such as would be produced by adding a quantity not of red but of *violet* light; and for the present I am not disposed to assume that white light contains an excess of violet light. The explanation offered by Aubert does not undertake to account for the changes produced in colours other than ultramarine, and even in this case seems to me arbitrary; neither have I succeeded in framing any explanation in accordance with the theory of Young and Helmholtz which seems plausible.—Silliman's *American Journal*, August 1880.

ON THE ABSORPTION OF RADIANT HEAT BY GASES AND VAPOURS. BY MM. LECHER AND PERNTER.

The authors discuss the different methods previously employed in similar investigations, and in particular the arrangements employed by Tyndall, and the vapour-adhesion to which they are liable, and show from numbers given by Tyndall himself how important a source of error this may become. Tyndall's results often differ by 30 per cent., according as the whole or only half of the experimental tube was polished.

There is scarcely any other reason to be found for this difference than vapour-adhesion. That a condensation of vapour along the walls of the tube took place may be shown directly by comparing Tyndall's observations in which the vapour-pressure was measured directly, with those in which the experimental tube was filled with vapour by being repeatedly placed in communication with a flask containing the corresponding vapour in a state of saturation. We can conclude from the absorption observed what the pressure must be in the flask. We find thus for benzol-vapour, for example, a tension of two atmospheres, which corresponds to a temperature of 100° C. But as the temperature in fact was 11° C., the excess of vapour must have resulted from precipitated liquid on the sides of the tube.

The authors were led by these considerations, and by experience obtained from numerous failures, to adopt finally an arrangement in which the thermopile and source of heat were placed in the same vessel. The influence of currents of air is rendered imperceptible

by bringing the radiating surface suddenly to 100° C. by a stream of boiling water applied exteriorly.

Of results must be mentioned, in the first place, that the absorption of radiant heat by aqueous vapour, in opposition to Tyndall's results, is found to be excessively small. The authors show that the contradiction between this result and certain meteorological observations is only apparent.

The experiments made by Violle on Mont Blanc show that one metre of the air at the place of observation absorbed only 0·007 per cent. of the total radiation. If account is taken of the hygrometric condition of the air, and the difference in wave-length of the heat experimented upon, we arrive at the conclusion that, taking Violle's observations as correct, it would require a layer of 300 metres of saturated aqueous vapour at 12° to produce the absorption of radiant heat which Tyndall finds for a length of 1·22 metre. By this simple calculation, as well as by the experimental results obtained, we may regard Magnus's view as established—that aqueous vapour exerts hardly any absorption on radiant heat.

The rest of the numbers obtained for gases agree fairly well with those given by Tyndall (which of course is not the case with the *vapours* examined). At atmospheric pressure the intensity of the radiation through a column of 310 millims. length (the intensity of the incident radiation being 100) is for

Air	99·8
Carbonic oxide	93·3
Carbon dioxide	92·3
Ethylene	51·8

It appears, further, that there is no simple relation between the absorption and the pressure of the substances experimented upon; and the absorption, even for the radiation from a source of heat at 100°, is a selective one. Hence too much importance is not to be attributed to the relation which is pointed out in the present paper between absorption and chemical composition, which is as follows. After the absorption for vapours at equal pressure (that is, for an equal number of molecules) had been found by a graphical method, it was seen that the absorption-coefficients for radiation at 100° C. of the substances examined which belonged to the fatty series might be arranged somewhat as follows:—

I. Methyl-alcohol, formic acid, carbon monoxide, carbon dioxide, chloroform.
II. Ethyl alcohol (acetic acid), ethyl ether, ethylene.
III. Butyl alcohol.
IV. Amyl alcohol.

The absorption is nearly the same for the substances in each series, but rises rapidly as the percentage of carbon increases.

It seems to be otherwise with bodies belonging to other groups. Thus, for example, benzol, notwithstanding its 6-carbon atom, possesses a somewhat small absorption-coefficient. Whether this is a consequence of the different linking of these carbon atoms and the consequent modification of the mode of vibration, can only be de-

cided by a spectroscopic examination of the absorption.—*Berichte der kaiserlichen Akademie der Wissenschaften in Wien*, 1880, XVII. pp. 135–138.

SPECTROSCOPIC RESEARCHES. BY DR. G. L. CIAMICIAN.

The author has obtained the following results by the study of twenty elements :—

I. Carbon has two spectra, one of the first and one of the second order, in accordance with the general rule that each element possesses two spectra.

II. Amongst carbon-compounds, only cyanogen, carbon-monoxide, and acetylene have special spectra.

III. The spectra of the radicals cyanogen and carbonyl have a simple relation to the spectra of the first order of their components. The most refrangible end of the carbonic-oxide and cyanogen spectra is homologous with that of the carbon spectrum of the first order. On the other hand, the less refrangible half of the cyanogen spectrum is to be compared with that of the nitrogen spectrum of the first order.

IV. The spectra of the second order of carbon, boron, silicon, and aluminium are homologous. It is to be remarked that the less refrangible end of the spectra of silicon and aluminium has nothing corresponding to it in the spectra of carbon and boron, and is comparable with the less refrangible portions of the spectra of the elements of the oxygen group. Boron, silicon, and aluminium have also spectra of the first order which correspond to the spectrum of carbon of the first order.

V. The spectra of the first and second order of carbon and of magnesium are completely homologous.

VI. The more refrangible half of the homologous spectra of barium, strontium, and calcium is homologous with the magnesium spectrum.

VII. The spectra of the elements oxygen, sulphur, selenium, and tellurium are completely homologous amongst themselves, both as regards the more refrangible and the less refrangible halves.

VIII. The spectra of phosphorus, arsenic, and antimony are to be compared with the spectrum of nitrogen in the red portions; and in like manner only the less refrangible portions of the spectra of the halogens are homologous with the spectrum of fluorine.

IX. The less refrangible end of the spectra of silicon, aluminium, calcium, strontium, and barium is homologous with that of the spectra of the elements of the oxygen group; and those elements are more nearly comparable which form a horizontal line in Mendelejeff's table, *e.g.* sulphur, silicon, aluminium, calcium, selenium, strontium and tellurium, barium.

X. The more refrangible end of the spectra of chlorine, bromine, iodine and phosphorus, arsenic and antimony, is homologous with the more refrangible part of the spectra of the elements of the oxygen group; the elements sulphur, chlorine, phosphorus, bromine, arsenic and tellurium, iodine, antimony are most nearly comparable.

From these relations of homology of the elements, and on

account of the homology mentioned (III.) of the spectra of cyanogen and carbonic oxide with the spectra of their elements, we may conclude that *the ground of the homology of the spectra of the elements is to be sought in the mode of their composition.*

From this hypothesis, in accordance with Mendelejeff's laws, the following conclusions may be drawn :—

The spectra of the elements carbon, boron, and magnesium are completely homologous. Hence these three elements consist of similar material existing in different stages of condensation, which find expression in the displacement of the homologous lines. The atomic weights of boron and carbon are nearly the same; that of magnesium is $24=2\times12$.

The spectra of silicon and aluminium are homologous; and the more refrangible portion corresponds to the spectrum of carbon, the less refrangible portion to that of oxygen. Silicon consists, therefore, of carbon and oxygen, with the corresponding atomic weight $12+16=28$.

Aluminium contains carbon in the form of boron (perhaps of beryllium) and oxygen, as its atomic weight $27=11+16$ indicates.

The elements of the group of alkaline earths have spectra whose more refrangible part corresponds to the spectrum of magnesium, and the less refrangible part to that of the spectra of the elements of the oxygen group.

Hence calcium, strontium, and barium consist of carbon in the form of magnesium, and oxygen in the condensed forms of sulphur, selenium, and tellurium, corresponding to the atomic weights

$$Ca=24+16,\quad Sr=24+4\times16,\quad Ba=24+7\times16.$$

The elements of the oxygen group all consist of the same material, present in different stages of condensation, which find expression in the displacement of the homologous groups of lines in the spectrum.

The atomic weights of the elements of this series are

$$O=16,\quad S=16+16,\quad Se=16+4\times16,\quad Te=16+7\times16.$$

The halogens all consist of fluorine and oxygen in different forms of condensation; the atomic weights of the elements of this group express these relationships—

$$Cl=19+16,\quad Br=19+4\times16,\quad I=19+7\times16.$$

The spectra of the elements of the nitrogen group are homologous in their less refrangible portion with that of nitrogen, in their more refrangible portion with those of the elements of the oxygen group. Hence the elements of the nitrogen group consist of nitrogen and oxygen, in different stages of condensation, as is indicated by the combining weights,

$$N=14,\quad P=14+16,\quad As=14+4\times16,\quad Sb=14+7\times16.$$

Berichte der kaiserlichen Akademie der Wissenschaften in Wien, 1880, XVII. pp. 138–141.

ON THE ACTION OF HOLLOW IN COMPARISON WITH THAT OF SOLID STEEL MAGNETS. BY W. HOLTZ.

In experiments on magnetizing steel during its hardening, I had

come to the opinion that solid steel bars do not in general furnish good *permanent* magnets, because, on the one hand, the core absorbs much of the magnetizing force already during the magnetization, and, next, because the core *generally* is to be regarded as an armature *joining the two poles**. I consequently instituted some further experiments, with steel tubes, which appeared, on the whole, to confirm that view.

Now, as the manufacture of such steel tubes presents no difficulty at all, but rather they can be had in larger picees at considerably lower prices than solid bars (for the tubes need not be bored, but can be forged out of sheet-steel, since it is indifferent whether the wall is perfectly closed or not), it did not seem superfluous, in regard to the possibility of turning it to practical account, to draw a more exact comparison between the action of such and ordinary solid magnets.

Now it is true that in the meantime I have learned that Nobili ascertained that a small steel tube became permanently more powerfully magnetic than a solid rod of equal external dimensions. According to the brief report in Poggendorff's *Annalen*, xxxiv. p. 270 (1835)—the original memoir, unfortunately, not being accessible to me—the rod weighed 28·5, and the tube 16 grams; in spite of this the latter caused a deflection of a compass-needle of 19°, while the former produced one of only 9°·5. Still this experiment, apart from the dimensions of the pieces having been proportionally small and the want of a more exact determination of the magnetisms, has hitherto remained isolated. Hence I should nevertheless like to describe in a few words the results of my own experiments.

First I employed a rod and a tube $12\frac{1}{2}$ centims. long and 13 millims. in outside diameter. The thickness of the wall of the tube amounted to $1\frac{3}{4}$ millim. Both rod and tube were magnetized up to saturation. The magnetisms were tested by the oscillation method, the tube being loaded throughout its whole length with copper. The magnetism of the rod was to that of the tube as 1 : 1·6.

I next used a rod and a tube of 32 centims. length and 35 millims. external diameter, while the thickness of the tube-wall was the same as the above. I believe that here also both rod and tube were magnetized to saturation, since an extraordinarily great magnetizing force was at my disposal. The magnetism of the rod (tested as above) was to that of the tube as 1 : 1·5.

I had, in truth, expected a greater difference in the latter case than in the former, because the core was so much thicker, and this circumstance would necessarily have great influence; but I soon reflected that the length of the core (the imaginary armature) must also have played an essential part. Had I in the second case, with the same thickness of the rod, used only a length of $12\frac{1}{2}$ centims., I should in all probability have found a much greater difference.

Lastly I tried, besides, how much the magnetism of the tubes would be lessened if I employed a real armature, *i. e.* if I filled them

* Holtz, Wied. *Ann.* vii. p. 71 (1879).

along their whole length with soft iron. The result was, that the tubes under such circumstances scarcely obeyed the directing force of the earth. This last experiment demonstrates, perhaps most strikingly, how prejudicial to *permanent* steel magnets is the interior mass.

Postscript.—Six months having elapsed since the above-mentioned comparison (which took place shortly after the magnetizing of the rods and tubes in question), and the above communication having already been delivered to the editors of the *Annalen*, it recently struck me to compare the same magnets again, as it certainly was probable that hollow magnets would retain their magnetism better than solid ones. The result was such as to far exceed my expectations; for I found that now, with those larger magnets, the magnetism of the solid was to that of the hollow magnet as 1 : 2·5, and the magnetism of the solid smaller magnet was to that of the hollow one in the ratio of 1 : 2·9.

As, after this, more attention will probably be bestowed in future on the manufacture of hollow permanent magnets for practical use, I have resolved to devote to this subject a longer series of experiments, and will in due time beg leave to communicate the results.—Wiedemann's *Annalen*, 1880, No. 8, vol. x. pp. 694–696.

ON AN ELECTRODYNAMICAL PARADOX.
BY M. GÉRARD-LESCUYER.

Dynamoelectrical machines, of which the Gramme machine is the best-known type, are reversible; that is to say, if a current be passed through them, they give motion, and may serve for the transmission of force. Under the same conditions the same properties are possessed by magnetoelectrical machines with *continuous currents.* In this there is nothing new. But if the current produced by a dynamoelectrical be sent into a magnetoelectrical machine, a strange phenomenon is witnessed, which we will describe.

As soon as the circuit is closed the magnetoelectrical machine begins to move; it tends to take a regulated velocity in accordance with the intensity of the current by which it is excited; but suddenly it slackens its speed, stops, and starts again in the opposite direction, to stop again and rotate in the same direction as before. In a word, it receives a regular reciprocating motion which lasts as long as the current that produces it.

What is the cause of this phenomenon?

Evidently the motive current must change in direction; this is proved by introducing a galvanometer into the circuit. But how can this reversal of the current be produced while the velocity of the generating machine (steam-engine, water-wheel, &c.) does not vary?

Some extraneous cause, then, must arise to reverse the polarities of the inductors of the generating dynamoelectrical machine, so that this machine may immediately give rise to a current of opposite direction, which reverses the direction of rotation of the

receiving machine. We verify this reversal of the polarities of the inductors by placing near them a simple compass, the needle of which suddenly turns half round at each change of magnetization of the inductors.

Now we ascertain that these movements of the compass-needle coincide with those of the galvanometer; we may then be sure that the two phenomena are connected the one with the other, and so intimately that the one must be the consequence of the other.

But this explains nothing. Let us frame an hypothesis, and suppose for a moment that the receiving magnetoelectrical machine can, from some cause *which we will not investigate*, receive periodically an increase of velocity. Under the conditions of this hypothesis our receiving magnetoelectrical machine, instead of continuing to revolve under the action of the current to which it was at first submitted, would, in virtue of its *increased* velocity, give rise to a current of its own, which in its turn would traverse the dynamoelectrical machine. As this current would have the opposite direction to that proceeding from the generating dynamoelectrical machine, it would reverse the polarities of the inductors and give rise to a new current having the same direction as itself, which in its turn would reverse the direction of rotation of the receiving machine.

We have seen above that these effects are shown by the galvanometer and the compass; but if our hypothesis is true, this phenomenon will no longer be produced when, by any means whatever, we prevent the receiving magnetoelectrical machine from increasing its velocity: for this purpose the application of a brake suffices. Now, as soon as the brake comes into play the preceding effects disappear: the rotation of the machine continues constantly in the same direction; the needles of the galvanometer and the compass remain motionless.

What are we to conclude from this? Nothing, except that we are confronted by a scientific paradox, the explanation of which will come, but which does not cease to be interesting.

The experiment is very easy to carry out; it succeeds as often as we will. Nevertheless it is necessary to say that with a generating machine such as the ordinary Gramme, the inductors of which are of cast iron, the experiment is more delicate and requires certain conditions of velocity, though very simple after all. We believe that this is due to the nature of cast iron, the residual magnetism of which offers a certain amount of resistance to the reversing current proceeding from the receiving magnetoelectrical machine. On the other hand, any machine with a soft-iron inductor, taken as generator, permits the first attempt to be successful, without any precaution or care.

We usually employ for the experiment a Siemens dynamoelectric machine with continuous currents as generator, and a small laboratory Gramme with ordinary permanent magnet as receiver.—*Comptes Rendus de l'Académie des Sciences*, July 26, 1880, t. xci, pp. 226, 227.

THE

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[FIFTH SERIES.]

OCTOBER 1880.

XXXI. *Note on the Laboratory at St. John's College, Oxford.* *By* R. H. M. Bosanquet.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In this note I propose to give a short account of the arrangements which have been carried out in the laboratory placed at my disposal by St. John's College. I shall not on this occasion enter upon any investigation. I may, however, mention that work has been begun, and that results have been already obtained which will appear in due course.

The laboratory in question had its origin in a paper which appeared in the Philosophical Magazine in October 1879 (fifth series, vol. viii. p. 290). This paper was laid before the College, with the view of obtaining permission to use some small out-of-the-way place as a beginning, and of getting a small grant in aid of the expenses. There happened to be a detached lecture-room, nearly new and solidly built, possessing qualities which made it unpopular as a lecture-room. This was placed at my disposal. A grant of £50 was given towards the expenses, and gas and water to be laid on.

The room in question is about 30 feet by 15 feet; it stands on a solid floor of concrete covered by a thin wood pavement. The front wall is of stone; the other walls are brick. The room was better adapted to its purpose than any place I had hoped to secure; and I determined to fit it up in a way that

should justify the confidence of the College. I then sent in an application to the Government-Grant Committee; the application resulted in a grant of £152. I propose now to give a slight account of the progress of the various arrangements.

Gas and Water.—It was found that it would be necessary to have a direct gas-supply, independent of the general supply of the college. The water-supply was brought in through the same route. The gas-main was laid through the concrete of the floor as far as the meter; and incidentally it was thus learned how hard that concrete is. The gas-main is $1\frac{1}{2}$ inch in diameter throughout. Subsequent experience has shown that it might have been somewhat larger with advantage. The laying of these pipes occupied nearly two months, and was a most severe trial to the patience of the neighbours. The meter is a 50-light meter. It is placed in the porch. The main running to it nowhere appears above the floor until just before it reaches the meter, a most necessary precaution in case of accidents. A 50-light meter is supposed to be able to pass 300 cubic feet of gas per hour; it will pass somewhat more without difficulty.

The water-supply is only an inch pipe. I considered the possibility of employing a water-engine; and if the Oxford water-works had been conducted in the modern fashion, so that a fair pressure could have been reasonably depended upon, I should have unhesitatingly preferred the water-engine as a source of power; but under the existing circumstances the water-service is not available for these purposes. The inch pipe already available was therefore thought sufficient. The details of the water-arrangements are hardly worth dwelling on, though they are elaborate and took a long time to carry out.

Steam-engine and Boiler.—Though the hot-air engine mentioned im my original paper would have been sufficient for some purposes, yet it possesses very small power; and with the improved site I determined to employ a steam-engine. The gas-engine is far more economical than a steam-engine for small powers; and if I had had a second room, I should certainly have employed a gas-engine. As I was situated, having only the one room at my disposal, I was obliged to seek for some mechanism that should be as nearly as possible noiseless. I selected a steam-engine mainly on this ground—a Willan's engine, which, although intended primarily as a high-speed engine for steam-launches, yet runs very smoothly at low speeds. The boiler, with the engine, feed-pump, and a surface condenser, are all bolted down to a cast-iron base-plate, which rests on the floor; and there is little vibration.

The arrangement of the surface condenser serves the double purpose of avoiding scale in the boiler (the Oxford water being heavily charged with lime) and of economizing steam, by letting the condensed water be pumped into the boiler again at a high temperature. The Willan's engine is not economical in steam; and its power is greater than is required for the main purposes of the laboratory. It answers fairly for the original purpose of blowing the bellows; and very well for such purposes as grinding tools and cutting the teeth of wheels on the lathe, particularly the latter, which is heavy work. But I have come to the conclusion that it is a mistake to have the motor in the room at all when acoustic experiments are being carried on. There is always some noise. In the case of a similar arrangement being fitted up again, it should be regarded as almost a necessity to have a separate room, or, if possible, a separate building, for the motor; and then a gas-engine would be admissible. This is out of the question in the same room, on account of the noise.

On another occasion, however, in publishing the results of the first investigations made in the laboratory, I shall show that results have been obtained with the combination of engine and bellows as originally designed. In all cases in which very minute analysis by the unassisted ear is not involved, this combination is quite satisfactory.

The exhaust-steam pipe is fitted with a three-way cock just above the condenser, from which a direct exhaust is led into the chimney, so that the system can be worked as a high-pressure or condensing arrangement at will. But the condensing arrangement is always used by preference, as being both more economical of steam and more quiet. The high-pressure exhaust causes a slight hum in the chimney, which, though hardly observable under ordinary circumstances, interferes to some extent with acoustic observations. No puffing is observed from outside with the high-pressure exhaust; only a light cloud of escaping steam rises from the chimney.

The engine was got into position about the beginning of April. The arrangements of the gas-furnace and condenser, however, were very imperfectly finished by the makers in the first instance; and it was not until the middle of June in the present year, after all these things had been subjected to a thorough revision, that the whole arrangement could be said to be in working order. A few days after the condenser was put right (it had been left so that the water ran through the joints of the tubes like a sieve) I had the pleasure of showing to Prof. Wiedemann the combination of steam-engine and pneumatic bellows, with self-acting control of the steam-engine

from the bellows, acting in a way that left nothing to be desired.

Shafting.—One of the most unpleasant pieces of work that had to be done was the fitting of the supports for the shafting into the walls. The back wall, being brick, was dealt with without great difficulty; but the front wall is of hard stone, and cutting the necessary holes right through it was a serious piece of work. What with the laying of the gas- and water-pipes and this, the laboratory was in the occupation of masons &c. pretty nearly the whole of last winter. The shafting is in two lines, each nearly 30 feet long. The two lines are connected by a belt about the middle of each. The pulleys are for the most part split pulleys, which can be put on and taken off without disturbing the shafting. This is an arrangement of the greatest convenience; indeed, without it no rearrangement, such as is often inevitable, would be possible without serious expense.

The lathe is by Messrs. Cook, of York. It has five-foot bearers, back gear for reduced speeds, a long screw (with change wheels) for the slide-rest, special apparatus for drilling, and cutting the teeth of wheels, and two new arrangements of a special character, which were carried out by Messrs. Cooke according to my directions.

The Micrometer.—This consists of a worm-wheel of 180 teeth on the pulley of the mandrel, in which a tangent-screw can be made to engage. The novelty consists of the details of the arrangement by which the turns of the tangent-screw can be subdivided. A number of gun-metal disks are provided, which fit on the tangent-screw. These have different numbers of holes pierced in circles near the rim. Pins are provided which can be inserted in the holes; a long arm carrying an inverted V, or perhaps rather a W, in metal, drops over the pin and defines its position accurately. Pointers are provided, which can be adjusted so as to indicate without trouble the hole in which the pin is to be inserted. The pin is inserted into the hole; the screw is turned through so many turns and the fraction over; the W is dropped over the pin last inserted, and the other pin placed in the hole indicated by the pointer. This is done after a little practice with great expedition and certainty. I have now divided several circles, and cut several wheels with all sorts of numbers of teeth, and have only, to my knowledge, made one mistake with the micrometer, which was detected at once in time to be remedied.

The micrometer possesses 20 wheels, whose numbers are as follows:—

26	32	38	46	53
28	34	41	47	59
29	36	43	49	61
31	37	44	50	67

The number of divisions thus obtainable is very large. If n be the number desired, then $\frac{180}{n}$ must consist of an integer and a proper fraction whose denominator is a factor of one of the numbers of the above table. The integer is the number of whole turns; and the fraction, reduced to one of which the number in the table is the denominator, gives the number of holes which represents the fraction of a turn.

Examples.

$$74 \ldots \frac{180}{74} = 2\tfrac{16}{37}.$$

$$125 \ldots \frac{180}{125} = 1\tfrac{22}{50}.$$

It is also possible to approximate to prime numbers by a rapidly converging process.

Example.

$$71 \ldots \frac{180}{71} = 2\tfrac{38}{71};$$

$$\frac{180}{72} = 2\tfrac{1}{2};$$

$$\frac{38}{71} - \frac{1}{2} = \frac{5}{142} = \frac{1}{28\cdot 4};$$

so that, if we represent

$$\frac{180}{72} \text{ by } 2\tfrac{14}{28},$$

we have

$$\frac{180}{71} \text{ nearly } 2\tfrac{15}{28}.$$

In fact

$$71 \times 2\tfrac{15}{28} = 180\tfrac{1}{28},$$

and the division errs by about $\frac{1}{70}$ of a division. But we should not often get so close as this; and the strength of the method lies in the next step.

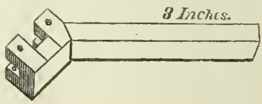
If by the above process we divide a micrometer-wheel with a number of holes such as 71, there will be a small error distributed about the wheel, having a maximum at one point. Now, if we use this in turn as a micrometer-wheel, the resulting division will in many cases be sufficiently accurate for all

practical purposes. For (1) the error can only affect each resulting division once, *i. e.* it does not appear in the entire turns; and (2) the effect of the error on a given division is divided by 180. So that, if x be the maximum error of the micrometer-wheel, $\frac{x}{180}$ is the maximum error of the resulting division. In the above case this furnishes a division abundantly accurate for ordinary purposes.

Arrangement for Turning large Disks and Circles.—A pair of small bearers is provided, standing in the middle of the lathe-board at right angles to the principal bearers. The lathe-head can be lifted off its place and put down on these small bearers. The mandrel is then at right angles to the principal bearers, and distant from them 2 or 3 inches. In this position disks or circles up to 4 feet in diameter can be attached to the mandrel and turned, the slide-rest travelling on the principal bearers along the face of such disks or circles. By aid of this arrangement I turned and divided the circles of my precessional globe, which I have since exhibited to the Royal Astronomical Society.

The lathe was said to be ready for delivery in January of the present year. On account of the masons' work then going on, I was unable to receive it at that time. Ultimately, when it was delivered, it turned out that some of the fittings had been forgotten, and others needed revision; so that the lathe was not entirely fit for use till well on in the spring. The acoustic arrangements not being as yet in a state of forwardness, I thought it legitimate to devote some time to the production of, first, the small model precessional globe, and afterwards the larger precessional globe, which I required for the investigations in ancient astronomy with which I have been concerned. This, with the preparation of chucks and tools of various kinds for the lathe, and the construction and adjustment of the electromagnetic dial hereinafter described, occupied me from the time that the lathe was ready up to June in the present year.

Among the tools constructed at this time is one which I think it worth while to describe more particularly, as I have never seen any thing like it, and its uses are manifold. I could not have divided the globe, or turned any of the larger work done on the lathe, without it.



It is a solid bar of iron, 1 inch square, expanded at one end into a tool-holder, the length of which is at an angle of 30° with the length of the bar. There are screw-holes

both above and below in the tool-holder; so that it can be placed either as shown or the other way up. Where large work is to be done on a lathe of moderate dimensions this will be found invaluable.

Since June the lathe has been used, chiefly in conjunction with the engine, in cutting change-wheels for Donkin's harmonograph, the set sold with the machine being quite insufficient for the production of the curves required for discussions having some relation to my first results.

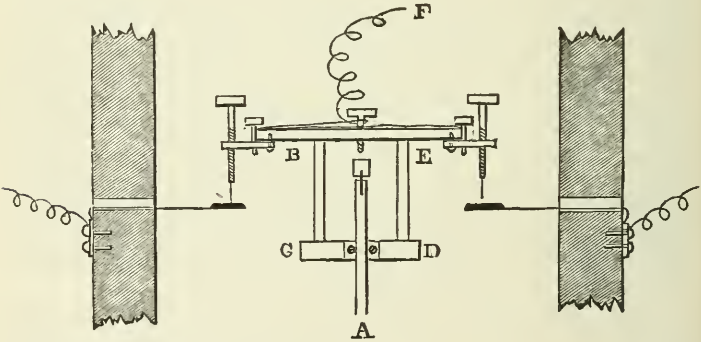
Pneumatic Bellows.—The bellows has been constructed on a somewhat larger scale than that originally contemplated. Every increase in the size of bellows is known to conduce to the steadiness of the wind. This bellows is about 6 feet long and 3 feet broad. It occupies the lower portion of a frame, the upper part of which carries a wind-chest supplied by wind-trunks of great capacity, and a large and solid table to place apparatus upon. Thirty slides altogether are provided, with apertures of various diameters, from nearly two inches to small fractions of an inch. The bellows itself consists of a reservoir formed with direct and inverted rib, with connecting guides, in the ordinary English style. This is supplied by three feeders, driven by three cranks set at angles of 120° on a shaft which runs the whole length of the instrument. The shaft carries a handle at one end, and a pair of fast and loose pulleys of large size at the other, by means of which the bellows can be driven from the shafting or disconnected at pleasure. The bellows was required to be able to deliver wind at a pressure of two feet of water. This is a very unusual test for organ-bellows; and it has not proved convenient to put sufficient weight on so large a bellows; but the bellows has worked for some time at a pressure of about 1 foot of water, which is amply sufficient for any thing likely to be required. The pressure of ordinary organ-wind is about $2\frac{1}{2}$ inches; and it will probably be convenient to work this bellows at from 3 to 4 inches for most purposes.

When the bellows is blown by the steam-engine, a lever is carried from the top of the reservoir to the regulating-handle of the engine. When things are properly adjusted, a wind of the most perfect evenness is obtained. It is, however, not practicable to leave the entire control of the steam to the bellows; in that case the combination would be overgoverned and oscillations would ensue. It is best to regulate the steam pretty exactly at the cock on the steam-pipe; and then the handle of the engine itself, which is attached to the regulating lever, has not too great an influence.

With reference to the employment of wind for the evalua-

tion of mechanical equivalents, I employ a 30-light gas-meter, through which the wind passes on its way from the bellows. This is connected with one of the largest slide-holes by means of a large indiarubber tube and boxwood connexions. In this way the wind used can be measured with considerable accuracy.

Electromagnetic Dial.—When the gas- and water-pipes were laid down, I took the opportunity to have a couple of electric wires, covered with gutta-percha, laid from my rooms to the laboratory. The object of this was to make a connexion from an excellent astronomical clock, which I possess, to the laboratory. I tried first a modification of a method recently applied at Greenwich, according to which light steel springs are acted on by a wheel on the spindle of the scape-wheel. I tried the employment of springs in connexion with the scape-wheel itself. This method proved an entire failure, owing to the tendency which the springs possessed to "hang up." I could not understand this for a long time, and thought that possibly magnetism might be involved; but it turned out to be only a property of the particular steel employed, which "buckled" occasionally, and set itself in its displaced position instead of returning to its proper position of rest. Eventually I adopted the following plan, which has proved so successful that it leaves nothing to be desired.



A is the pendulum-rod.

B C D E a wood frame, embracing the rod tightly at the middle of C D, and having its top bar B E above the suspension of the pendulum.

The bar B E carries at its extremities two brass plates arranged as shown, both kept in communication with the return-circuit by means of the wire F. Two large gun-metal screws are carried in these brass plates, having stout platinum wires inserted into holes drilled in their axes. From the clock-

case projects on each side a horizontal spring, carrying a bit of platinum-foil just under the platinum termination of the screw. These springs are of phosphor-bronze, a material recommended to me by Dr. Siemens. I dare say other materials would answer; but these have certainly been faultless in my hands; and, as I have said, the steel was by no means so. The horizontal springs communicate with the wires which are laid to the laboratory. The return-circuit is taken through the gas- and water-pipes.

This arrangement permits the adjustment for beat to be made with great accuracy. I have watched the dial going with these contacts continually, and never knew them miss. The electric communication, being once laid on from the clock, is of course available for all purposes.

The arrangement of the electromagnetic dial is simple. Two electromagnets stand opposite each other, and pull alternately at a framework of brass and soft-iron armatures suspended pendulum fashion, within which is a ratchet-wheel of thirty teeth. Two bits of phosphor-bronze spring, attached to the inside of the framework, impel the ratchet-wheel through half a tooth at each second. The spindle of the ratchet-wheel carries a hand which shows seconds on a dial. The same spindle carries an endless screw, which gears in a worm-wheel of sixty teeth. The spindle of this latter wheel carries a hand which shows minutes on another dial. The arrangement, though simple, answers its purpose completely. The only difficulty with it arises in connexion with the use of Leclanché cells: these rapidly lose their strength when set to continuous work of this kind. I have hitherto managed by keeping a number of them and changing those in use from time to time. The whole of this little machine was made in the laboratory.

The electromagnets were wound, and the wheels and spindles cut, on the lathe.

Metal-casting Arrangements.—I thought it would be useful to be able to do castings on a small scale; and I employed one of Fletcher's gas-furnaces in the first instance. Brass was given up, on account of the intolerable nuisance caused by the fumes of the burning zinc; but gun-metal worked very well. I procured some moulding-sand, and flasks or boxes of different forms. I cast a number of gun-metal rings for the spring chucks that were made for wheels, as also the circles for the small model of the precessional globe. But I found that I should frequently require larger plant; and considering that I had only the one room, and the mess that is made by the cleaning, wetting, drying, and other treatment of the sand and loam used, I thought it best to give up the casting, at all

events as an ordinary thing. It may come in useful occasionally when things are wanted in a hurry.

Emery Grinder for Tools.—This is an excellent machine of American make, small, cheap, and very efficient. It is screwed down upon the filing-bench, where it occupies hardly any space, and is driven from the shafting above. The tools ground by this machine are quite a different thing from tools ground on the ordinary grindstone, and they take a small fraction of the usual time to grind. A few minutes before or after the regular work of the engine suffice to keep the tools in such order as could hardly be attained otherwise by an occasional half day at the grindstone.

The carpenter's bench is a common one. The filing-bench is of great solidity, let into the wall and floor. It supports two vices and the emery grinder.

I will not now enter into any details as to the acoustic apparatus, but leave that for a subsequent occasion.

St. John's College, Oxford,
July 29, 1880.

XXXII. *On the Electric Resistance of Glass at different Temperatures.* By THOMAS GRAY, *B.Sc., C.E., Demonstrator in Physics and Instructor in Telegraphy, Imperial College of Engineering, Tokio, Japan**.

IN the following paper are described the results of some experiments on the electric resistance of glass at different temperatures. These experiments were undertaken with the view of finding whether the variation of resistance with temperature followed a law sufficiently definite to allow the resistance at low temperatures, of highly insulating glass, to be approximately calculated from its measured resistance at high temperatures. My chief object was to use this law, with results obtained by the galvanometric method of measuring resistance, for the comparison of specific resistances of different kinds of glass.

Published results of experiments on various substances, such as gutta-percha, india-rubber, glass, &c., as well as some experiments of my own on other substances, led me to expect that the resistance of glass might possibly be expressed by an equation of the form

$$\log R = C - C't,$$

where R is the resistance of the material, t the temperature, and C and C′ constants depending on the nature of the mate-

* Communicated by the Author.

rial. I believe that some such law as this will be found to hold for all dielectrics when the effects of the permanent changes produced in the glass by heating are eliminated. These changes are, I find, of considerable magnitude in the case of glass; and it is to them especially that I desire to call attention in the present paper.

With the exception of one experiment, in which a Thomson's quadrant-electrometer jar was used, the whole of the experiments, the results of which are detailed below, were made on ordinary test-tubes, which had lain in our store-room for several years, but which had not been used for any other purpose. The resistances were measured by the direct deflection of the needle of a very sensitive astatic galvanometer, produced by the current passing through the glass. The resistance of this galvanometer was 10,000 ohms; and one Daniell's element produced a deflection of one millimetre on the scale when a resistance of $9{\cdot}96 \times 10^9$ ohms was interposed in the circuit.

The battery most commonly used consisted of 10 Daniell's elements; but at low temperatures a battery of 110 Daniell's cells was sometimes used. I was thus enabled to measure with a somewhat close approach to accuracy a resistance equal to 2×10^{14} ohms per cubic centimetre in my test-tube experiments, and equal to 14×10^{14} in my electrometer-jar experiment. The test-tube glass had generally a resistance considerably below the former figure even at low temperatures; but the electrometer jar reached the latter figure at a temperature of about 100° Cent.

The advantage in these experiments of using this galvanometric method of measuring the resistances, rather than the method with electrometer and condenser of great capacity, was the ease with which the temperature and resistance could be simultaneously observed.

In determining the resistances two methods of observation were employed. One consisted in passing the current through the glass first in one direction and then in the opposite direction, and taking the deflection in each case half a minute after the battery had been applied. The mean of these deflections was then taken as the deflection corresponding to the resistance to be measured; and the mean temperature was taken as the temperature of that particular determination.

The second method consisted in passing the current continuously through the glass during the whole experiment, and taking deflections by short-circuiting the galvanometer.

The first method gives the resistance before any very great amount of polarization has taken place, at least before more

than is proper to that temperature has taken place. The second method gives during the heating pretty nearly what is believed by some to be the true resistance ; but it also shows whether more or less polarization takes place when the temperature is high than when it is low. It therefore gives some indication of the effect of the current flowing constantly through the glass while its temperature is being changed.

The results which I have obtained agree with the well-known result that the resistance of a piece of glass diminishes as the temperature is raised ; but they also show that the resistance varies more slowly when the glass is cooling than when it is being heated, and that a piece of glass may have its resistance greatly increased by being slowly raised to a high temperature and slowly cooled. There seems, therefore, to be what may be called a permanent change in the quality of the glass, which takes place coincidently with a temporary or quasi-elastic change in the quality; and a comparison of the results obtained when the glass was cooling with those obtained when it was being heated, showed that some such equation as that which I have stated above probably expresses this latter change. Whether the former change is really permanent or not (that is, whether the glass retains this increase of specific resistance at low temperatures) I am not yet in a position to say with certainty ; but, from observations which I have made, I am inclined to think that slow changes take place in glass, tending to raise its conductivity and also its specific inductive capacity.

The following table, the results in which were obtained by the second method described above, gives the resistance at four different temperatures, in ohms per cubic centimetre, of a test-tube during four successive heatings and coolings. $H_1, H_2, H_3, H_4, C_1, C_2, C_3, C_4$ indicate the first, second, third, and fourth heatings and coolings respectively.

Temperature.	H_1.	C_1.	H_2.	C_2.
60°	20×10^{10}	24×10^{10}	25×10^{10}	44×10^{10}
100	79×10^{8}	10×10^{9}	81×10^{8}	22×10^{9}
150	23×10^{7}	30×10^{7}	27×10^{7}	50×10^{7}
200	18×10^{6}	18×10^{6}	20×10^{6}	30×10^{6}

Temperature.	H_3.	C_3.	H_4.	C_4.
60°	40×10^{10}	27×10^{11}	13×10^{11}	70×10^{11}
100	18×10^{9}	11×10^{10}	38×10^{9}	41×10^{10}
150	50×10^{7}	30×10^{8}	12×10^{8}	14×10^{9}
200	67×10^{6}	70×10^{6}	16×10^{7}	73×10^{7}

The resistances in columns C_3 and C_4 are too high, because of excessive polarization; but taking H_1, H_2, H_3, and H_4, we find that the effect of the first three heatings has been to raise the resistance to six times its original amount. Again, by reversing the battery at the end of the experiment C_4, I found the resistance at $60°$ about 35×10^{11}; that is to say, the resistance had been increased eighteenfold by the four heatings.

In the electrometer-jar experiment the results were similar. The resistance in this case, however, can very nearly be expressed by the equations:—

$$\log R = 17{\cdot}535 - 0{\cdot}322\,t$$

when the temperature is rising;

$$\log R = 18{\cdot}1248 - 0{\cdot}0353\,t$$

when the temperature is falling.

The heating in this experiment was much more slow than in the test-tube experiment: three hours was the time occupied in raising the temperature from 100° to 200° C. It is possible that the greater regularity of the results in this case is due to the slower heating, which would admit of the permanent change being more nearly accomplished for each temperature before it was passed. The resistance at 113° C. at the end of the experiment was twice that at the beginning; and by continuing the curve given by the equation down to 0°, a resistance of four times the original amount was indicated, part of which must have been due to polarization. For this glass, then, the so-called permanent change of resistance produced by heating and cooling is comparatively small; but probably a greater effect would have been produced if the jar had been raised to a higher temperature.

When the glass is being heated, the polarization-effect produced by keeping the current constantly flowing through the glass is very slight at low temperatures: only a very small apparent increase of resistance can be observed after several minutes' electrification. As the temperature is raised, however, the polarization gradually increases; and this increase remains to a considerable extent during the cooling. The increase of polarization is not due to the length of time the battery has been applied before the temperature is raised; and it assumes its former value within two or three minutes if the battery is reversed at the high temperature.

The following table shows the result of a set of four experiments on a test-tube. The first two columns give the temperature and resistance when the glass was being heated, the third and fourth columns the same when the glass was cooling. The measurements were not, however, made at so low tempe-

ratures as I could have wished; but they are sufficient to show the general effect to which I wish to direct attention. In the first two experiments the readings were taken according to the first of the two methods described above; in the other two the second method was followed.

Experiment I.

Temperature.	Resistance.	Temperature.	Resistance.
		67°	14×10^{10}
69°	98×10^{10}	96·5	127×10^{9}
110·5	34×10^{8}	109	48×10^{8}
127·5	92×10^{7}	128·5	13×10^{8}
151	25×10^{7}	150	30×10^{7}
173	68×10^{6}	169	67×10^{6}
194·5	22×10^{6}	194·5	22×10^{6}

Experiment II.

Temperature.	Resistance.	Temperature.	Resistance.
15°	12×10^{12}		
25	57×10^{11}		
35	26×10^{11}		
43	12×10^{11}	38°	27×10^{11}
48	72×10^{10}	47·5	136×10^{10}
59	27×10^{10}	58·5	47×10^{10}
78·5	47×10^{9}	76·5	12×10^{10}
100·5	79×10^{8}	97	25×10^{9}
120	18×10^{8}	116	57×10^{8}
140	48×10^{7}	137	136×10^{7}
163	12×10^{7}	160·5	29×10^{7}
198	16×10^{6}	196	37×10^{6}
220	57×10^{5}	210	20×10^{6}
244	28×10^{5}	237	52×10^{5}
*243·5	40×10^{5}		
258	38×10^{5}	258	38×10^{5}

Experiment III.

Temperature.	Resistance.	Temperature.	Resistance.
94°	24×10^{9}	93°	20×10^{10}
99	169×10^{8}	108	57×10^{9}
136	121×10^{7}	128	124×10^{8}
163	226×10^{6}	157	181×10^{7}
183·5	169×10^{6}	176	52×10^{7}
205	57×10^{6}	195	79×10^{6}
214	53×10^{6}	214	53×10^{6}

* The temperature was maintained about 244° for an hour: at the end of that time the glass showed a marked increase of resistance.

Experiment IV.

Temperature.	Resistance.	Temperature.	Resistance.
74°	37×10^{10}	83°	14×10^{11}
98	43×10^{9}	98	45×10^{10}
127	57×10^{8}	128	60×10^{9}
147	163×10^{7}	147	19×10^{9}
166·5	48×10^{7}	166·5	53×10^{8}
185·5	18×10^{7}	184	204×10^{7}
201	98×10^{6}	204	57×10^{7}
220	51×10^{6}?	214	46×10^{7}
225	51×10^{6}	224	27×10^{7}
246	27×10^{6}	230·5	19×10^{7}
*247	57×10^{6}	247	57×10^{6}

The battery was reversed at the end of this experiment; and the following resistances were then obtained:—

80°	31×10^{10}
77	57×10^{10}
74	85×10^{10}
71	115×10^{10}
68	160×10^{10}

The following table of results, extracted from an unfinished series of experiments on the resistance and specific inductive capacity of porcelain, may be interesting:—

Resistance, in ohms, per cubic centimetre.	Temperature.
12×10^{12}	68°
86×10^{11}	73
27×10^{11}	86
68×10^{10}	103
12×10^{10}	130
30×10^{9}	150
94×10^{8}	170
35×10^{8}	185
20×10^{8}	200
60×10^{7}	220
23×10^{7}	250
97×10^{6}	270
44×10^{6}	290
73×10^{6}	270
56×10^{7}	220
19×10^{8}	200
41×10^{8}	185
92×10^{8}	170
47×10^{9}	144
15×10^{10}	125
74×10^{10}	100

* The temperature was kept about 247° for half an hour before the reading for resistance was taken.

These results show a variation considerably smaller than would be given by the logarithmic curve derived from the results at low temperatures. So far as my experiments have gone, it seems as if in the case of porcelain a slight increase of resistance were produced by the first heating, and a diminution by subsequent heatings. This result, however, requires further verification; possibly heating to a certain temperature produces increase, and heating to a higher temperature diminution, of resistance.

I made also some rough determinations of the specific inductive capacity of the glass and porcelain before and after the heatings to which they were subjected, and also some attempts to measure it at various temperatures while the glass was being heated and cooled. Although my experimental arrangements* were not such as to give absolute results with accuracy, those obtained at low temperatures may fairly be compared with one another.

As these determinations were merely preliminary, I think it unnecessary to enter into particulars regarding them, and content myself with referring to a more extended series of experiments which I am carrying out with improved apparatus and experimental arrangements, and an account of which I hope to publish before long. So far as my present results go, they show that the combined effect of heating and cooling is to diminish the specific inductive capacity of glass. Some experiments made on a Thomson electrometer-jar seemed to show a temporary or quasi-elastic increase of specific inductive capacity with heating and diminution by cooling; but this result requires verification by more numerous experiments.

The following table of results, which show the effect of heating a test-tube to 240° C. in a sand-bath and allowing it to cool slowly, illustrates what has been stated above. The numbers in the first column are proportional to the specific inductive capacity of the test-tube; and those in the second column give the temperature:—

51	12°

Heated to 240° and allowed to cool slowly,

46	23°
55	57
50	40

These figures show a diminution of about 13 or 14 per cent.

* The arrangement was one of a Wippe or rapid commutator, with a ballistic galvanometer through which the condenser was discharged. The speed of the Wippe, however, was not sufficiently great.

in the specific inductive capacity, and also that this quality is much affected by change of temperature.

The following numbers give the results of a corresponding experiment on the resistance of the same tube. The numbers in the left-hand column are proportional to the conductivities, those on the right give the corresponding temperatures:—

12	12°

Heated to 240° and cooled slowly,

9	23°
300	57
55	49

Judging from these results, the resistance of the tube at 12° at the end of the experiment must have been about three times what it was at the beginning. This tube was then heated to redness in a blowpipe-flame, and cooled quickly by blowing air against it. The capacity deflection was then found to be 52 at 30° C., and the conductivity deflection 16 at the same temperature. It was then heated to 250° and allowed to cool slowly as before. The results were then found to be 52 divisions for capacity and 18 for conductivity. This shows a slight *diminution* of resistance, due to the heating to 250° immediately after the glass had been heated to redness and cooled quickly. It appears, therefore, that the permanent change is not due to annealing.

These results seem to point to the conclusion that slow changes take place in glass which render it a better conductor of electricity, and at the same time increase its specific inductive capacity*. I am not sure that any considerable deterioration in the insulating-power of glass stems and electrometer-jars has been detected; but in such a case these experiments point to an at least temporary cure—keeping the glass at a high temperature for a considerable time. This accords with the treatment sometimes adopted for badly-insulating ebonite.

* This conclusion is confirmed, so far as the capacity is concerned, by the observations of Mr. J. E. H. Gordon on change of specific inductive capacity in pieces of glass which had been kept carefully for a considerable time after their specific inductive capacity had been first determined ('Nature,' Sept. 18, 1879).

XXXIII. *On the Electric Discharge in Rarefied Gases.—Part II. On Luminous Phenomena in Gases caused by Electricity.* By Dr. Eugen Goldstein*.

[Concluded from p. 190.]

On a new Phosphorescent Action of the Electric Discharge.

THE phosphorescence caused by the kathode-rays has been hitherto the only example of the excitation of luminosity on solid surfaces caused by an infinitely thin layer of the discharge, and therefore forming well-defined images. The images are the section of the bundle of electric rays by the wall of the tube.

I have succeeded during the last year in discovering two other modes of exciting such phosphorescence, and of uniting previous observations into a more extended and consistent research, whose results are of universal applicability.

The first of these new methods of producing phosphorescence occurs equally with pressures at which the kathode-rays excite the previously-known phosphorescence and with pressures 1000 times greater—equally at atmospheric pressure and when the pressure is $\frac{1}{100}$ millim. This mode of phosphorescence may be observed by surrounding one electrode of a vacuum-tube with a phosphorescent powder, which fills the space between the electrode and the tube and also reaches beyond the free end of the electrode. If, then, while both electrodes are in connexion with the induction-coil, the outer surface of the glass in the neighbourhood of the powder be touched with a conductor, star-like discharges pass from the conducting body to the surface of the glass, similar to those observed, in producing Lichtenberg's figures in the dark, on the non-conducting plate to which the point conveying the electricity is opposed.

Besides these exterior discharges, there are also others between the inner wall and the surface of the mass of powder in contact with it in the neighbourhood of the point touched. These discharges also are branched; but in general they show a much richer ramification and more beautiful dendritic forms.

Now these interior discharges cause phosphorescence in the surface of the mass of powder; but this phosphorescence is not equally distributed over the surface, but forms a pattern of surprising delicacy, in which are seen accurate reproduc-

* Translated from a separate impression, communicated by the Author, from the *Monatsberichte der königlichen Akademie der Wissenschaften zu Berlin*, January 1880.

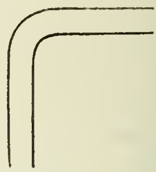
tions of all the ramifications of the discharge recognizable by the eye. This phosphorescent drawing shows moreover a marvellous number of finer ramifications, which the eye is not able to trace in the discharge itself which produces them. As this phosphorescent light produced by the discharge is much brighter than the light directly emitted by the discharge, we are probably right in supposing that the phosphorescence in the fine ramifications represents parts of the discharge whose light is too weak for their direct observation, in the study of which, therefore, the production of phosphorescence renders useful aid. I hope to be able to show, later on, that the study of such branching brush-discharges is absolutely necessary to a more intimate knowledge of the *κατ' ἐξοχὴν* electric spark (so-called) and of forked lightning.

The green phosphorescence produced in finely-powdered glass by such discharges was recognizable as the pressure was gradually reduced, when the pressure reached 50 millims.; and with pulverized calc-spar, it showed itself in a magnificent orange-red pattern, even at atmospheric pressure. As the pressure of the gas diminishes, the brightness of the phosphorescent light increases, and at the same time the figures extend over greater surface and increase in fineness and richness of detail. Instead of exciting this phosphorescence by connecting both electrodes with the induction-coil and touching the outer surface by a conductor, they may be produced even in greater perfection by disconnecting the wire from the electrode not surrounded by the powder, and bringing it, instead of the conducting body, into contact with the outer surface of the glass in the neighbourhood of the powder. We observe in this case that the luminous figures possess a different habit according to the polarity of the touching wire. These are amongst the most beautiful of the luminous phenomena produced by electricity.

The second mode of phosphorescence is less striking in the form in which it appears, but leads to important conclusions on the nature of the so-called positive light. Whilst the kathode-light radiates in inflexibly straight lines, it appears as if the positive light always consisted of bundles of more flexible rays following each bend of the discharge-tube—if indeed we employ the word ray at all for so great an enfeeblement of the properties of negative light.

This view is decisively opposed by experiments which I have made during the last year, at least for the *positive light produced in gas of very small density*. When the positive light fills a highly exhausted tube which is bent at any point (without alteration of section, see woodcut) we observe as follows:—

At the bend, on the side of the tube which forms the convexity of the bend, there appears a *bright phosphorescent surface.* The surface is a half ellipse, or, if no boundary on the one side is to be detected, of parabolic form. The axis of the parabola lies in a plane which would cut the bent tube lengthwise into two halves, meeting at the bend. The surface is sharply bounded about the vertex, which is turned towards the positive end of the tube. At the opposite side turned towards the kathode it gradually loses itself and presents an uncertain boundary. If we call the greatest extension of the surface at right angles to the axis measured on the circumference of the tube its breadth, then its breadth is somewhat less than the half circumference of the tube. The sharply-bounded end of the surface reaches a little further towards the positive side than the line in which the leading lines of the inner surface of that branch of the bend which is on the negative side would cut the other branch if produced.



If, now, we employ a tube with several bends instead of one, then we observe a phosphorescent surface of similar properties on the convex side of *each bend*. From this it follows that the phosphorescence is *not caused* by the rays of the kathode: these could at most only produce phosphorescence at the first bend, but in consequence of their rectilinear propagation could not reach beyond it ; hence the phosphorescence is produced by the *positive light itself.*

The phosphorescence of the surface at the bend is produced, like that caused by the kathode-light, by a *very thin layer lying in close contact with the wall of the tube.* This follows from the sharp boundary which the surface shows at the end turned towards the positive end of the tube ; and it follows also from the fact that insulated wires suitably placed near the bend cast sharp shadows on the phosphorescing part of the wall of the tube. This last phenomenon shows, at the same time, that the electric motion which manifests itself in the light has a *regular radiation.*

If instead of *one* wire two be placed to throw shadows, which both lie in a plane coincident or parallel with the mesial plane of the tube, their shadows are found to be superposed. Hence it follows that this regular radiation of the positive light is *rectilinear.* The position of the shadow shows that the phosphorescence is produced by rays very nearly parallel to the axis of the tube, which radiate *from the side of the kathode towards the side of the anode.*

That the rectilinear direction of radiation does not com-

pletely coincide with the direction of the axis of the cylindrical portion of the tube follows from the previously-mentioned fact, that the surface extends towards the positive side a little beyond the section of the negative branch of the tube by the positive branch. Observations made with a tube to be described further on, show that the deviation of the rays from the direction of the axis is equal in every direction; whence it follows that the rays of the positive light are not parallel to each other, but form a conical bundle of small angle whose section is circular.

We therefore regard the phosphorescent surface in the bends as the luminous sections of the column of light which traverses the negative branch of the bend by the wall of the tube ; and we have arrived at the following general view of the positive light in highly exhausted cylindrical tubes:—

The positive light in highly rarefied gases consists of rectilinear rays which are propagated from the negative to the positive side. The rays form a conical bundle of small angle whose axis is the axis of the cylindrical portion of the tube; where this bundle cuts the *wall of the tube*, the parts of the rays in *immediate* contact with the wall excite *phosphorescent light* in it.

The analogy which these properties show exists between the rays of the positive light and the kathode-rays is obvious. I have inquired whether this analogy does not go so far as to amount to a negation of the characteristic property hitherto always ascribed to the positive light, according to which it invariably takes the *shortest* course from the boundary turned *towards the kathode* to the anode. Although the kathode-light radiates its rays without reference to the position of the anode, the path of the positive light is essentially conditioned by the position of the anode.

Tubes of the form of fig. 10 (Pl. IV.) served for the trial whether the behaviour of the positive light corresponded in fact with the usual assumption. The whole vessel, with the exception of the ellipsoidal bulbs A and B, is made out of the same piece of glass tubing. Care was taken that there was no change in section at the points α, β, and γ, where the tubes joined.

In a particular case the tube was 1 centim. wide, the distances between the points α and β and β and γ was 6 centims., and the "blind alley" prolongations of the tube X, X, X had each a length of 2 centims. from the rectangular junction.

If the tube is highly exhausted and B be made the kathode, the discharge presents the following appearance.

The positive light (lilac at this pressure, and blue at the greatest exhaustion possible) extends from B through the branch 1 of the tube till its rectilinear rays impinge on the

wall of the first bend, and excite a green phosphorescence at the point b, of the form already described. Close behind the bend the positive light shows an obliquely-placed maximum of light, which is sharply bounded on the negative side and falls off very rapidly, whilst on the positive side the brightness decreases very gradually.

The positive layers traversing the branch 2 reach the branch 3 at the point γ, but do not at once bend round in the direction of 3, but continue as far as the wall opposite to the opening γ as a well-defined conical bundle of small angle parallel to the axis of 2; the section of this part of the tube by the bundle issuing from γ appears as a bright sharply-defined green phosphorescent surface.

Near the bundle there is in 3 a new maximum of positive light, separated from it by a feebly-illuminated interval. This maximum, like the first, is convex towards the negative side, its brightness falling off rapidly on that side, gradually on the positive side, thus possessing exactly the cup-like appearance of the boundary of one of the stratifications of positive light turned towards the kathode: its axis also coincides with the axis of the branch 3. The blind prolongation X near γ remains *completely empty and shows no light*. Precisely the same appearances are seen at β and α: here also well-defined bundles issue, which continue as far as the wall opposite the opening, and produce there, where they cut it, sharply-bounded areas of phosphorescence. After each bundle at β and α there follows a positive maximum symmetrically placed with reference to the axis of the tube. After the bundle which produces a green surface at the bend a, there follows again an obliquely-placed maximum of positive light. The blind prolongations X all remain empty and dark. If now the direction of the current be reversed, the discharge presents the following form (fig. 12):—The positive light which begins at the opening of 6 produces a green surface at the bend a, the sharply-bounded edge of which is now turned in the opposite direction (towards 5, instead of towards 6 as in the previous case). Behind the bend, again, there is an obliquely-placed maximum of positive light, which falls away sharply on the negative side; the positive rays in 5 do not bend round at the point α into the tube 4, but continue with decreasing intensity into a prolongation X, which they completely fill. At the *closed* end of X is seen *green phosphorescence*; but there is now *no* phosphorescence at the part of the tube 5 lying opposite the opening α. At the entrance of 4 there is, again, a cup-shaped positive maximum, above which and distinctly separate passes the bundle in 5. The same appear-

ances are observed at all the corresponding points of the tube. As indicated in the figure, the positive light at β and at γ does not take the shortest way to the anode by avoiding the prolongations X, but fills them and produces green phosphorescence *at their ends.* There is no phosphorescence opposite β and γ; there is none opposite α.

The experiments show, then, that the positive light, like the negative light, with increased exhaustion radiates *in straight lines as far as the shape of the discharge-tube allows; it fills every space which can be reached in the direction of its rays* without traversing a solid wall, even when the path to this space and to its boundary deviates from the shortest path to the anode.

On Crookes's *Theory of the Phenomena of the Discharge.*

In the Philosophical Magazine for January 1879 Crookes has proposed a theory of the excitation of phosphorescence by electric rays, which brings this excitation into close relationship with the second (counted from the surface of the kathode) layer of the kathode-light. Crookes believes that the discharge from the kathode consists of electrically-charged particles of gas driven off from the surface of the kathode. These particles of gas drive the uncharged molecules before them to a certain distance from the kathode; and there results a space round the kathode filled only with these molecules driven off from the kathode. As their paths are normal to the surface in the case of a straight wire or a plane surface of metal, the paths would be all divergent or all parallel, and the molecules would not suffer collision with each other. But, according to Crookes, the mutual collision of molecules is the only cause of their luminosity; consequently this space surrounding the kathode appears dark. This space expands with increase of exhaustion in all directions until its diameter becomes equal to the distance of the kathode from the wall; then the molecules thrown off from the surface of the kathode, before they collide with other gas-molecules, strike upon the wall of the tube and excite it to luminosity.

In opposition to the above I have to remark:—

1. The second layer of the kathode-light cannot consist of non-luminous molecules driven off from the kathode; for the kathode is surrounded immediately by the *bright* yellow-coloured *first* layer. It would be an error to consider this layer as a secondary glow produced by volatilized sodium; for its spectrum is that of *air* free from sodium-lines.

2. The second layer itself is also not *non-luminous*, but distinctly of a blue colour; at the small density of the luminous gas this involves a very *high* emission-coefficient.

3. The rays of the kathode-light are *rectilinear* both within the third layer and within the second of a straight kathode-wire; but Crookes's theory implies that the discharge, and consequently the repulsive electrification, at the kathode must continue at least until the first repelled molecules have traversed the diameter of the second layer; whence it would follow that the rays, at least within the second layer, must be *hyperbolic*, and not straight. For so long as the repulsive electrification continues, the repelled molecules must follow *lines of force*; and their form is determined by the condition that about a straight thin wire equipotential surfaces form confocal ellipsoids.

Although the above has long been known to me, yet, in order to further test Crookes's theory, I have made additional experiments, which I will briefly describe.

In a cylinder the kathode was formed by a plane surface of metal, obliquely to which was placed a phosphorescent plate, so that at the densities at which the discharge excites phosphorescence a part of this plate lay within and a part without the second layer of the kathode-light. But as, according to Crookes, the second layer (in opposition to the rest of the kathode-light) consists only of molecules not in collision, and the phosphorescence of the wall is caused only by the impact of such not yet intercepted molecules, the phosphorescent plate ought to show a sharply-marked contour-line where the plate is intersected by the outer surface of the second layer, on one side of which it should be brightly phosphorescent, and on the other dull or altogether non-luminous. Observation shows, however, that no such dividing line exists; the brightness of the phosphorescent plate alters continuously from point to point, and is still of considerable intensity in the neighbourhood of the outer kathode-light, which, according to Crookes, consists almost entirely of colliding particles incapable of exciting phosphorescence.

Mr. Crookes must have observed that in particular cases surfaces lying outside of the second layer still phosphoresce. To explain this he assumes that a few molecules have much overpassed the mean free path of the molecules thrown off from the negative pole, and so reach and excite phosphorescent screens lying outside the second layer.

The mean free path of the molecules thrown off from the negative pole agrees, according to Crookes, with that calculated from the kinetic theory of gases for the mean free path in the gas contained in the discharge-tube. I had a cylindrical tube constructed, 90 centims. long, in which the kathode was a plane surface of metal at one end of the tube and at

right angles to its axis. When the second layer had attained a thickness of 6 centims., the opposite end of the tube phosphoresced brilliantly under the influence of negative rays reaching so far.

According to experiments of Mr. E. Hagen, Assistant in the Physical Institute of Berlin, the smallest density attainable by the use of a mercury pump of the construction which I employed is $\frac{1}{125}$ millim. of mercury. If we assume that this smallest possible density was reached in my experiment, then the corresponding mean free path, assuming the value given by Maxwell for atmospheric pressure, would be only $0{\cdot}00006 \times 760 \times 125$ millims. $= 5{\cdot}7$ millims. But the above observation shows that the actual distance of the second layer is more than ten times as great.

Inasmuch as a surface almost 90 centims. distant from the kathode phosphoresced brilliantly at this pressure, a considerable number of molecules must have passed through a distance about 150 times as great as the mean free path. The probability of this for a single molecule would be e^{-150} or about 7×10^{-66}. The discharge-tube has a capacity of about half a litre. According to Thomson a cubic centimetre of air at ordinary temperature and pressure contains about 3×10^{20} molecules. In our tube at the given density there would be about 2×10^{17}. The probability that even a single one of the molecules thrown off from the kathode should reach the end of the tube without previous collision, is therefore a vanishing quantity. The value of this probability is still less if we take account not only of the density of the residual gas, but also of the tension of the mercury-vapour present.

Mr. Crookes has in various publications expressed the opinion that the kathode-rays always radiate at right angles to the surface from which they are emitted. As a proof of this, he describes an experiment in which a small spherical concave mirror is employed as kathode, and in which the phosphorescent surface on the wall of the tube is reduced to a *point* when the wall coincides with the centre of curvature of the mirror. I was convinced from my own experiments that this statement of Crookes's could not be correct; and from observations which I had previously made with variously curved kathodes, I suspected that the experiment cited as proof by Crookes had not been completely described.

In order to verify my suspicion I made experiments with a concave spherical mirror as kathode, whose aperture was $21\frac{1}{2}$ millims. and radius of curvature $12\frac{1}{2}$ millims. The result was, that when the mirror was placed in the position with

reference to the wall of the tube chosen by Crookes, under certain circumstances the phosphorescent surface of the kathode-light was reduced to a point ; but *without alteration of the position of the mirror* it was possible, either by alteration of the density of the gas or by interpolation of sparks in the discharge, to obtain *surfaces of considerable and very various diameters as foci of the mirror*, instead of a point. I venture, for the sake of clearness, to give a few data of the experiments.

The distance of the centre of aperture of the mirror from the wall was 15 millims. ; a small Ruhmkorff's coil which gave sparks about $1\frac{1}{2}$ centim. long in air was discharged through a cylindrical tube of 4 centims. width, whose axis was cut at right angles by the axis of the mirror. At a pressure of about $\frac{1}{6}$ centim. there appeared as focus a bright round phosphorescent disk of 4 millims. diameter. If there were now interposed in the external circuit an air-spark of varying length, the diameter of the phosphorescent disk *increased* as the interpolated air-spark increased in length. The diameter of the surface (measured by means of a divided strip of paper laid on the outside of the tube) increased thus up to 1 centim. At $\frac{1}{12}$ millim. pressure the diameter without the spark was $2\frac{1}{2}$ millims., and when the spark was interpolated rose to 8 millims. At $\frac{1}{24}$ millim. the smallest diameter of the image was $1\frac{1}{2}$ millim.

When, instead of the small Ruhmkorff apparatus, a larger inductorium, giving much longer sparks, was employed, the diameter of the surface varied even when the position of the mirror and the density of the gas remained unaltered, even with the *same* length of interposed air-space. (It is probable that this variation is connected with the fact that sparks of different length may leap across an air-space of constant length, in consequence of the curvature of the sparks, the more strongly-curved sparks representing greater tensions.)

Thus with constant air-space and constant pressure the diameter of the luminous surface varied rapidly from 2 millims. to more than 1 centim.

In another case the centre of the mirror was at a distance (25 millims.) from the wall equal to twice the radius of curvature of the mirror. According to the law of divergence of the rays supposed by Crookes, the phosphorescent surface ought to have had a diameter equal to that of the mirror ($21\frac{1}{2}$ millims.), and ought to have remained constant.

At a pressure of $\frac{1}{2}$ millim. the kathode-light produced no phosphorescence when the exterior circuit was altogether metallic ; but when sparks were interposed, phosphorescent surfaces up to 26 millims. diameter were obtained.

At a pressure of $\frac{1}{4}$ millim. the largest surface obtainable by interposition of sparks had a diameter of 22 millims. (measured as before, on the circumference of the tube).

At a pressure of $\frac{1}{8}$ millim. a phosphorescent surface (whose diameter amounted to 12 millims.) was obtained even without sparks; by interposition of sparks the diameter was increased up to 19 millims.

Pressure $\frac{1}{16}$ millim. Without sparks the diameter varied between 9 and $11\frac{1}{2}$ millims.; with sparks it rose to 14 millims.

Pressure $\frac{1}{32}$ millim. Without sparks, diameter 7–8 millims.; with sparks, 10 millims.

Pressure $\frac{1}{64}$ millim. Without sparks, diameter 7 millims; with sparks, not perceptibly greater.

The size of the focal surface therefore increases in this case, as it does generally, when a spark is introduced and when it is lengthened. As the density of the gas decreases the magnitude of the surface obtainable with metallic circuits decreases; and at the same time the magnitude of the maximum diminishes, to which the diameter of the surface can be brought by interpolation of sparks, and more rapidly than the magnitude of the surfaces obtained with metallic circuits. The amplitude within which the magnitude of the surfaces varies becomes thus continually smaller as the evacuation is continued, until at very small densities the diameter of the surface remains constant and is not affected by the interpolation of sparks. It might be supposed that the change in the magnitude of the surface occupied by the ends of the rays emitted by the mirror is only an apparent one; that the surface itself is actually of the same size, but that with different degrees of exhaustion and intensities of discharge the brilliancy of the phosphorescence excited is different; if now the intensity of the surface is not uniform, but decreases from the centre, zones of the surface nearer the outside might become visible when the intensity of the phosphorescence was greater than when the intensity was less, in which case only the inner portions were bright enough to be seen. The increase and diminution of the focal surface thus reduces itself to an increase or diminution of intensity.

With reference to such objections, it must be noticed that the brilliancy of the surface decreases only very slightly from the centre to the periphery, but, *with each magnitude of the surface, falls away very rapidly at the outside,* so that the boundary of the surface is sharp and clearly marked. Further experience shows that a decrease of the surface takes place when the density of the gas is reduced, whereas,

as is well-known, the diminution of the density causes an increase in the brilliancy of the phosphorescence. But an appearance which is decisive on this point, results from a slight imperfection in the mirror employed; the mirror, struck with a die and subsequently polished, was not perfectly smooth near the edge (in consequence of imperfect malleability of the iron out of which it was made), but showed there short slight wrinkles. In consequence of this, the phosphorescent disk does not appear bounded by a smooth curve, but the periphery of the image shows small teeth and protuberances.

If, now, in consequence of interpolation of sparks or change of density, the image alter its diameter, *the same teeth and projections appear at the corresponding points in the same relative positions,* only larger when the surface is larger and smaller when the whole surface has diminished.

It may then be considered definitely proved that the observed variation in the magnitude of the surface corresponds to an actual change in diameter of the section of the bundle of rays emitted by the kathode by the glass-wall; or *that the direction of the rays emitted by a concave kathode is not constant, but varies with the density of gas, and with the change of conditions caused by intercalating sparks.*

It is on account of phenomena of this kind that, in various papers on the radiation of the kathode-rays and the intensity of the phosphorescence produced, I have distinguished the cases of convexo-concave and plano-convex kathodes from those of concave kathodes.

It has long been known that, in accordance with Doppler's principle, the velocity of translation of the luminous gas-molecules in the discharge must influence the spectrum of the gas. As it appeared to me that the experimental treatment of this point might furnish a new criterion whether the discharge consists of a convective transport of electricity by the gas-molecules or not, I was glad to avail myself of the opportunity offered me by Prof. Helmholtz to make use of a spectroscope of powerful dispersion. I constructed a discharge tube, in which the electrodes were two flat pieces of metal at right angles to each other. The connexions could be rapidly changed, so as to make either of the two pieces the negative pole. The tube contained rarefied hydrogen. The part of the tube containing the kathode was so placed in front of the slit of the spectroscope that the axis of the collimator was at right angles to the surface a, and consequently parallel to the surface b. The direction of the bulk of the rays emitted by a coincided with the axis of the collimator; the rays from b were at right angles to the colli-

mator-axis. Now, if, the electric rays consist of gas-molecules whose motion in the direction of the axis constitutes the propagation of electricity, and if the electricity in the ray propagates itself with the velocity c, the wave-length of the rays of light of the kathode-light emitted by a must appear smaller than the wave-length of the rays of light belonging to b, in the proportion of 40,000 to 40,000 $+c$ miles. The lines of the spectrum of a, in comparison with the corresponding maxima of b, must therefore appear displaced towards the violet end of the spectrum; or, if we suppose that the light from a contains some molecules moving more rapidly or less rapidly than the rest, then at least the lines of the spectrum belonging to a must show an expansion towards the blue. The observations were made on the bluish-green line of the hydrogen-spectrum (F in the solar spectrum); and the result was that, on interchanging the electrodes, there was neither displacement nor broadening of the lines large enough to be certainly observed; or, more exactly, there was neither a displacement nor a broadening of H_β (the F line) amounting to a third of the distance between the two sodium lines in the same apparatus. I was satisfied with this (for my purpose) sufficient result, without making further measurements of the dispersion of the apparatus. It indicates that the velocity of displacement of the gas-molecules does not amount to 14 miles per second (a more accurate knowledge of the relative dispersion in the different regions of the spectrum would, undoubtedly, considerably reduce this value); the dispersion in the neighbourhood of F would be at least $1\frac{1}{2}$ times as great as in the neighbourhood of D; the velocity of the gas-molecules may therefore be taken as less than 10 miles.

Wheatstone observed the image of a vacuum-tube nearly 2 metres long, in a revolving mirror whose axis of rotation was parallel to the tube, and observed whether the reflected image of the tube placed itself obliquely to the axis of rotation. Such a result would have led to a conclusion as to the time required by the discharge to pass from one end of the tube to the other. The tube remained parallel to the axis of rotation, even with a velocity of 800 revolutions per second; and by determining the limit at which an oblique position of the tube would have been noticed, the experiment gives as the velocity of propagation of the electricity through a vacuum at any rate a large multiple of 10 miles.

I am at present occupied with the preparation of experiments similar to those of Wheatstone's, and hope to be able to make independent experiments in this direction. In the

mean time I have endeavoured to draw conclusions as to the velocity of propagation of electricity in kathode-rays from certain phenomena which I have observed, and have obtained, as results, very great velocities. I venture here, shortly, to indicate the method of precedure. If two kathodes, a and b, are placed beside each other in a vacuum-tube, each of them shuts off certain rays from the other from a particular portion of the glass wall*. There result therefore, at suitable densities, two phosphorescent surfaces which appear dark in comparison with the surrounding parts of the glass wall, inasmuch as the one receives no rays from a and the other no rays from b, whilst the surrounding portion of the glass receives rays from both. If, now, the rays of the kathode a, for example, were not *permanently* cut off, but alternately cut off and allowed to pass during equal small intervals, this would be easy to observe.

It would only be necessary to emboss the kathode a and to leave b smooth; then in the dark surfaces to which the rays from a have only alternately access, the pattern of the electrode a would be observed †. Its brightness would, of course, only be the half of that which would be observed if a could always send its rays without hindrance to the surface in question; but the half of this illumination is, as experiments by way of control show, amply sufficient. Experiments with relief-kathodes show *permanent exclusion* of the rays from a, even when the two kathodes are at a distance of 20 centims. from each other. Hence it follows that when the discharge from a reaches b each time, the discharge from b has not yet lasted half its time, or the velocity of propagation of the discharge in the kathode-rays is great enough to pass over the interval between the two electrodes within half of the duration of a discharge. But now the duration of the partial discharges which constitute a (break) discharge is, according to experiments of mine with a revolving mirror, less than $\frac{1}{2,000,000}$ second; so that the velocity of the discharge is at least $2 \times 2,000,000 \times 20$ centims. or 800,000 metres. It cannot be objected to this conclusion that, as the discharges are separated from each other by numerous very small intervals of time, a discharge issuing from a in the electric ray might perhaps not reach b until, with the arrangement described, the discharge simultaneously commenced at b had long ceased and another producing a similar effect had begun. If this were the case, then when the distance between the kathodes was varied, a point would be reached at which the discharges which a sends out

* *Monatsber. d. Akad.* 1876, p. 285. † *Ibid.* p. 286.

would reach b during the intervals between the discharges emitted by b. But as, according to all experimental evidence, these intervals have even greater duration than the discharges themselves, the whole light from a would gain access each time to the dark surface, and the dark surface would, contrary to experience, vanish altogether.

This result, combined with that of the spectroscopic experiment, shows again the improbability of the convective action in the discharge, against which hypothesis I have already adduced experimental evidence in the paper laid before the Academy in the preceding year.

Berlin, Oct. 20th, 1879.

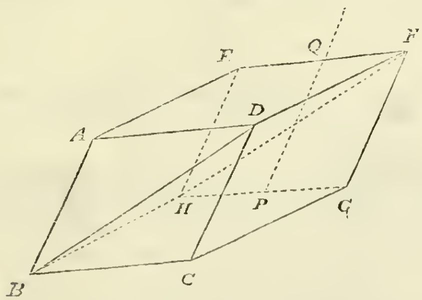
PM Series V. 10. (1880)

XXXIV. *Notes on Nicol's Prism. By* R. T. Glazebrook, *M.A., Fellow of Trinity College, and Demonstrator of Physics in the Cavendish Laboratory, Cambridge**.

In using a Nicol's prism to measure the rotation of the plane of polarization of light, two adjustments are necessary: (1) the incident light should be parallel to an edge of the rhomb of spar which constitutes the prism; (2) the axis of rotation of the prism should also be parallel to this edge.

I have lately found it requisite to discuss the effects that errors in these adjustments would produce in the results of some experiments I have been engaged in; and it appeared that the discussion may be useful to others working with a Nicol's prism.

Let A B C D E F G H be the prism, A B C D being the face



of incidence. Let the incident-wave normal be parallel to A E, and suppose, for simplicity, that A B C D is a rhombus.

* Communicated by the Author.

Join B D, and let C be the angle of the rhomb through which the optic axis passes.

A C G E is a principal plane; and B D is perpendicular to it. Hence B D is a possible direction of vibration in the rhomb.

Again, B D is clearly parallel to the intersection of the incident wave and the face A B C D. Both the refracted waves therefore cut A B C D in a line parallel to B D. Thus B D is the direction of vibration in one of the refracted waves, while that in the other is perpendicular to B D.

Let us consider that wave in which the direction of vibration is parallel to B D. It cuts the face E F G H in a line parallel to F H or B D; and this line is also parallel to the direction of vibration.

The emergent wave cuts the face E F G H in the same line; and the direction of vibration in it will be also parallel to this line. The emergent light is polarized, therefore, in a plane parallel to the principal plane of the prism. Had we taken the other wave, the emergent light would have been plane-polarized in a perpendicular plane.

But now suppose that the incident light is not parallel to A E. The incident wave will no longer cut A B C D in a line parallel to B D.

Let the wave-front in the crystal cut the face E F G H in a line P Q.

P Q is not generally a possible direction of vibration. The direction of vibration in the crystal is inclined to P Q at an angle (θ' say); that in the emergent wave will be inclined at another angle (θ suppose). Let ϕ' be the angle of incidence, ϕ that of emergence; then, if the medium were isotropic, we should have the equation

$$\tan\theta' = \tan\theta\cos(\phi - \phi'). \quad . \quad . \quad . \quad . \quad (1)$$

This follows from the electromagnetic theory of the reflexion and refraction of light (Phil. Mag. April 1880, page 290), as well as from Fresnel's.

Now experiments made recently by myself appear to show that the same law holds very approximately for Iceland-spar when there is only one wave transmitted in the spar*. Thus, assuming this formula to hold, the emergent light is plane-polarized; but its plane of polarization no longer coincides with that of the light in the prism. The two are inclined at an angle equal to that between the directions of vibration,

* Since writing the above, I have shown that, on the electromagnetic theory, this equation expresses the condition that one wave only should be propagated in the crystal.

while the plane of polarization of the light in the prism is no longer fixed relative to the prism.

Consider a sphere, centre O. Let O N be parallel to P Q, the intersection of the wave-fronts and the face E F G H; O V′, O V parallel to the directions of vibration in the two waves. Then N O V, N O V′ are parallel to the wave-fronts,

$$\mathrm{N\,V}=\theta, \quad \mathrm{N\,V'}=\theta', \quad \mathrm{V\,N\,V'}=\phi-\phi'.$$

Also since

$$\cos \overline{\phi-\phi'}=\cot\theta\tan\theta',$$

N V′ V is a right angle, and

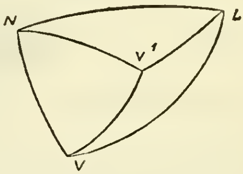
$$\sin\theta'=\tan \mathrm{VV'}\cot(\phi-\phi').$$

V V′ is the angle between the planes of polarization $=\chi$ (say); then

$$\tan\chi=\sin\theta'\tan(\phi-\phi'). \quad . \quad . \quad . \quad . \quad (2)$$

θ' is the angle between P Q and the possible direction of vibration in the crystal corresponding to a wave cutting E F G H in P Q.

To find this direction, draw a plane through B D perpendicular to the optic axis; the line in which it cuts the wave-front in the crystal either coincides with or is perpendicular to the direction of vibration. Take the two as coincident. As before, let O N be parallel to P Q, O V′ to the direction of vibration, O L to the line F H. Then N O L is the face of incidence, N O V′ the wave-front inside, L O V′ the plane perpendicular to the optic axis. Also



$$\mathrm{N\,V'}=\theta' \quad \mathrm{L\,N\,V'}=\phi'.$$

Let

$$\mathrm{N\,L}=\psi, \quad \mathrm{N\,L\,V'}=\alpha.$$

α is known, being the angle between a plane perpendicular to the optic axis and one of the rhombic faces. We can show that

$$\alpha=45^\circ \text{ approximately};$$

ψ depends on the direction of the incident wave, and can be found when that is given. And we have

$$\cot\theta'\sin\psi=\cos\psi\cos\phi'+\sin\phi'\cot\alpha. \quad . \quad . \quad . \quad (3)$$

We must therefore determine ψ.

Take, as axes of x, y, z respectively, a normal to A B C D

and the two lines A C and B D. P Q is parallel to the trace of the incident wave on A B C D. Let the equation to this wave be

$$lx+my+nz=0.$$

Equation to trace is

$$my+nz=0.$$

Equation to B D is

$$y=0.$$

Hence

$$\tan\psi=-\frac{n}{m}.$$

Now, when the wave-normal is parallel to A E, since the edge of the rhomb is inclined at about 20° to the normal to the face of incidence,

$$n=0,\quad l=\cos 20^\circ,\quad m=\cos 70^\circ.$$

Consider now a wave-normal in a plane through this and the axis of z inclined at, say, 5° to this. m will not be much altered, and the value of n will be about cos 85°. From these values we find

$$\psi=14^\circ \text{ about.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Also, taking the value of ϕ as about 20°, we have for mean rays

$$\phi'=12^\circ \text{ about.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

From this we find

$$\theta'=11^\circ\ 50'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Also

$$\phi-\phi'=8^\circ \text{ about.}$$

Thus, substituting in $\tan\chi=\sin\theta'\tan(\phi-\phi')$,

$$\chi=1^\circ\ 39'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Thus the planes of polarization of the light in the prism and the emergent light are inclined at an angle of 1° 39′ instead of being coincident.

Again, let us find the angle between the plane of polarization of the emergent light and that principal plane of the prism which is perpendicular to BD.

Let OV be the direction of vibration of the emergent light. Then NV′V is a right angle, and LV is the angle required.

$$NV=\theta,\quad LNV=\phi.$$

Let $\quad LV=\sigma,$

$$\cos LV=\cos NV\cos NL+\sin NV\sin NL\cos LNV',$$

$$\cos\sigma=\cos\theta\cos\psi+\sin\theta\sin\psi\cos\phi, \quad . \quad . \quad . \quad (8)$$

$$\tan\theta=\frac{\tan\theta'}{\cos(\phi-\phi')}.$$

Substituting the values from above, we find

$$\theta = 11^\circ\ 75', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

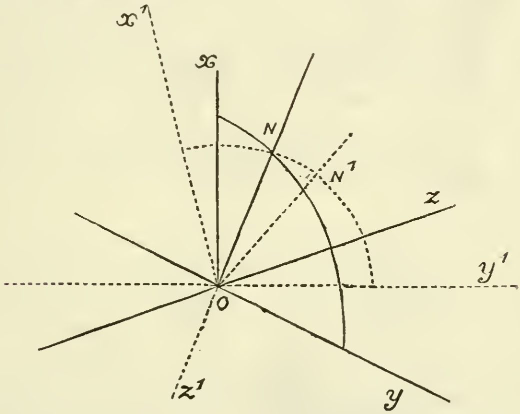
and

$$\sigma = 5^\circ\ 3'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Thus in this case the plane of polarization of the emergent light is inclined to a plane fixed in the Nicol at an angle of $5^\circ\ 3'$.

Let Ox, Oy, Oz be the three rectangular axes taken above, viz. the normal to the face of incidence drawn inwards, and two lines parallel to AC and BD respectively.

Let O N in the plane xOy be inclined at 20° to Ox. O N



is approximately parallel to an edge of the rhomb, and is the axis round which the rhomb is turned.

Let O N′ in the plane N O Z be inclined at 5° to O N. O N′ is the direction of the incident or emergent-wave normal in the case considered above; and the plane of polarization of the emergent wave is inclined at an angle of $5^\circ\ 3'$ to the plane xOy.

Now let us suppose the Nicol rotated through an angle of 90° about ON. Let Ox', Oy', Oz' be the new positions of the axes; then ON′ lies in the plane $y'Ox'$.

$$N'Ox' = 25^\circ, \quad N'Oy' = 65^\circ, \quad N'Oz' = 90^\circ.$$

Thus, if l', m', n' are the new direction-cosines of ON′,

$$l' = \cos 25^\circ, \quad m' = \cos 65^\circ, \quad n' = 0.$$

And in this case the intersection of the incident wave and the face of incidence is a possible direction of vibration; for it is parallel to the line BD. Also using the same notation as previously, ψ, χ, θ, θ', and σ are all zero.

The plane of polarization of the emergent wave coincides

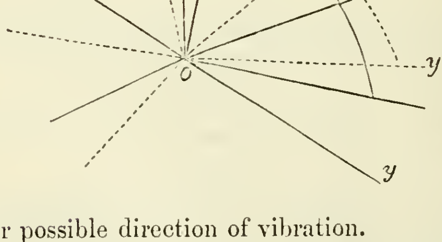
with $x'Oy'$. But previously the plane of polarization of the emergent wave was inclined at 5° 3′ to xOy.

Thus by this rotation the Nicol and the plane xOy have been turned through 90°, while the plane of polarization of the emergent light has been rotated through $90° \pm 5°\ 3'$. Thus, if the incident light were inclined in this manner to the axis of the Nicol at 5°, we might introduce an error of 5° 3′ into the measurement of a rotation of about 90°. The sign of the error will depend on the direction in which the Nicol is turned.

I have assumed that the axis of rotation of the Nicol is parallel to one edge of the rhomb of which it is composed. If the two are inclined at a small angle, the numbers will be modified slightly, but remain much of the same magnitude.

It seemed hopeless, from the number and complication of the formulæ, to try and solve the problem generally; I was therefore obliged to give numerical values to the quantities involved. I have supposed, too, that the Nicol has been cut directly from a rhomb of spar; so that the face of incidence is a cleavage-plane.

Let us now consider briefly the case in which the ends of the Nicol are perpendicular to its length, and therefore to the direction of the incident light. As before, take the normal to the face of incidence as axis of x; let a plane perpendicular to the optic axis cut the face of incidence in the axis of z, so that it is still a possible direction of vibration. The angle between this plane and the face of incidence will be greater by 20° than it was in the previous case; so that it is now 65°. Then Oy



is another possible direction of vibration.

Clearly, by supposing the incident-wave normal to lie in a plane midway between xOy and xOz, the intersection of the wave-front and the face of incidence may be inclined at 45°

to the possible directions of vibration; that is, with the same notation as previously,

$$\psi = 45°.$$

And if we suppose the axis of rotation to be inclined at 5° to the incident light,

$$\phi = 5°,$$

$$\phi' = 3° \text{ for mean rays.}$$

Also

$$\alpha = 65°.$$

∴ substituting in

$$\cot\theta' \sin\psi = \cos\psi \cos\phi' + \sin\phi' \cot\alpha,$$

$$\theta' = 42° \; 49',$$

$$\tan\theta = \frac{\tan\theta'}{\cos(\phi - \phi')},$$

$$\theta = 42° \; 50';$$

also

$$\cos\sigma = \cos\theta \cos\psi + \sin\theta \sin\psi \cos\phi,$$

$$\sigma = 4° \; 17'. \quad \ldots\ldots\ldots \quad (11)$$

Now suppose that the prism and axis of coordinates are turned round ON′ until the plane xOy passes through ON′. The amount of rotation will be rather more than 45°; and in this case we shall have

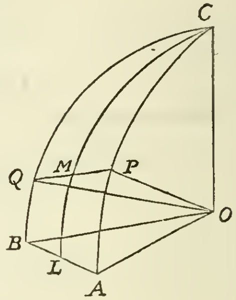
$$\psi, \theta, \theta', \text{ and } \sigma \text{ all zero.}$$

Thus the plane of polarization of the emergent light now coincides with the plane $x'Oy'$, whereas previously it was inclined at 4° 17′ to the plane xOy. Thus, in a rotation of about 45°, the motion of the plane of polarization differs from that of the Nicol by 4° 17′, or an error of 10 per cent. is introduced into the measurement. The error, therefore, in a prism with ends normal to its length may be greater than in one with oblique ends. As before, if the axis of rotation be oblique to the length of the prism the expressions would require slight modifications, but the numbers would remain of about the same magnitude.

But there is another way in which the measured rotation is affected by the inclination of the axis of rotation to the length of the Nicol. The angle we require is that through which a plane passing through the optic axis of the Nicol and bisecting one angle of the rhombic face of incidence has been turned. Unless the axis of rotation is parallel to this plane, the angle

through which the Nicol has been turned, as read by the circle attached, is greater than that through which the plane has been moved.

Let β be the angle between the axis of rotation and this principal plane. Owing to the rotation, the plane moves as tangent to a cone of semi- vertical angle β.



Let O P, O Q be perpendicular to two positions of the plane, O C the axis of rotation. Draw great circles C P A, C Q B, making C A and C B each quadrants. P O Q is the angle the plane has been turned through; A O B is that through which the Nicol has moved. Also

$$\mathrm{AOP}=\beta.$$

Draw C M L bisecting the angle A C B. Let

$$\mathrm{AOB}=\gamma, \quad \mathrm{POQ}=\phi;$$

$$\mathrm{AL}=\frac{\gamma}{2}, \quad \mathrm{PM}=\frac{\phi}{2}, \quad \mathrm{PC}=\frac{\pi}{2}-\beta.$$

For the right-angled triangle, PCM,

$$\sin \mathrm{PM}=\sin \mathrm{PC} \sin \mathrm{PCM},$$

$$\sin\frac{\phi}{2}=\sin\frac{\gamma}{2}\cos\beta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Let us suppose, as before, that $\alpha=5^\circ$, and that the rotation given by the circle is 90°, then $\gamma=90^\circ$. And from the above formula,

$$\phi=89^\circ\ 32';$$

and the error introduced is about one half per cent. Moreover the sign of the error is always positive; that is, the rotation as measured by the circle may be greater than its actual value by one half per cent. This, of course, is very small compared with the optical error introduced from obliquity between the axis of rotation and the direction of the incident light.

XXXV. *On the Employment of the Electrodynamic Potential for the Determination of the Ponderomotive and Electromotive Forces.* By R. CLAUSIUS*.

§ 1.

IN order to represent in a convenient manner the electrodynamic forces between moved particles of electricity and the mechanical work performed by them it is well known that the electrodynamic potential can be employed, which facilitates the calculations for these forces in like manner as the electrostatic potential does for the electrostatic forces. Its signification is the same as that of the electrostatic potential; for as the latter is defined by the statement that the work done during a movement of the particles of electricity by the electrostatic forces is equal to the simultaneous diminution of the electrostatic potential, so also the electrodynamic potential is defined by saying that the work done by the electrodynamic forces is equal to the diminution of the electrodynamic potential. In its form, however, the electrodynamic differs essentially from the electrostatic potential by this—that it comprises not only the coordinates, but also the components of the velocity of the electric particles; and with this is, at the same time, connected a difference in the procedure by means of which the force-components are to be derived from it.

If, now, we wish to determine with the help of the electrodynamic potential the forces which a *galvanic current* (which may be in motion and variable) exerts upon a moved particle of electricity, we cannot in general construct the former by simply combining, for each current-element, the two potential-expressions referring to the positive and negative electricity present in the respective element of the conductor in an algebraic sum and then treating the current-element as a whole, but must rather consider each of the two quantities of electricity separately, since the question is not merely what state of motion they have in the conductor-element which concerns us, but also how the state of their motion changes on their passing from this element into the adjacent one, which takes place differently for the two electricities. Of course the formulæ are thereby somewhat complicated. In certain cases, however, especially in that in which the current whose action upon a moved particle of electricity we wish to determine is

* Translated from a separate impression, communicated by the Author, from the *Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens*, vol. xxxvii. 1880. Read at the meeting of the Niederrheinische Gesellschaft für Natur- und Heilkunde on July 12, 1880.

closed, the thing is simplified in this way—that, besides the intensity of the current, we have only to consider the position and direction of the current-elements, without taking separately into consideration the two electricities present in them. Thereby we then arrive at formulæ of extraordinary simplicity, which afford great facilities for the determination of the ponderomotive and electromotive forces, and bring into clear view the entire field of mathematical developments having reference thereto.

These formulæ I will take leave to develop in the sequel—and not merely from the electrodynamic fundamental law advanced by me, but also from those of Riemann and Weber. It will be seen that the results of this formulation corresponding to the three laws differ from each other only by solitary, easily determinable terms, and hence can very conveniently be compared with one another.

§ 2.

Let a moved quantity of electricity, the amount of which does not affect the question, and which we will therefore take as a unit of electricity, be at the time t in the point x, y, z and have the velocity-components $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$. Given, further, a galvanic current s', which may likewise be in motion. For the sake of simplicity we will provisionally assume that the current is linear, since this involves no important limitation, as a current which is not linear can always be supposed to be divided into an infinite number of infinitely thin current-threads which can be regarded as linear currents.

Let us now contemplate in the conductor of the current, first, a single particle of the flowing electricity: this has a double motion—first, the motion of the current in the conductor, and, secondly, the motion of the conductor itself. In order to distinguish from one another the variations of the quantities which come into consideration occasioned by these two motions, we will, in like manner as I have already done in a previous investigation*, introduce the following method of designation. The coordinates of a fixed point in the conductor we consider simply as functions of the time t; while for the determination of the coordinates of the particle of electricity flowing in the conductor we take as an auxiliary a

* "Ueber die Behandlung der zwischen linearen Strömen und Leitern stattfindenden ponderomotorischen und electromotorischen Kräfte nach dem electrodynamischen Grundgesetze," *Verhandl. des naturhist. Vereins der preuss. Rheinl. u. Westf.* vol. xxxiii. 1876; Wied. *Ann.* vol. i., and Clausius, *Mechan. Wärmetheorie*, Bd. ii. Abschn. x.

second variable, which defines the situation of the particle in the conductor—namely, the distance s' (measured on the curve of the conductor) of the particle from any initial point. Accordingly each coordinate of the particle is to be regarded as a function of t, while, again, s' itself can be considered a function of t. If, then, x', y', z' are the coordinates of the electricity-particle at the time t, the complete differential coefficient of each of those coordinates with respect to t divides into two terms containing the partial differential coefficients with respect to t and s', so that we obtain for each coordinate an equation of the following form—

$$\frac{dx'}{dt}=\frac{\partial x'}{dt}+\frac{\partial x'}{\partial s'}\frac{ds'}{dt}.$$

For the differential coefficient $\frac{ds'}{dt}$, which represents the velocity of the currrent, we will introduce a simple symbol; we will denote by c' the velocity of the flow of the positive electricity, and by $-c'_1$ that of the negative electricity, while we then remain at liberty, according to the special assumption we make respecting the behaviour of the two electricities, either to consider the quantities c' and c'_1 equal the one to the other, or to put one of them $=0$, or to ascribe to them any values different from one another. With the aid of this notation we get, instead of the preceding equation, the two following, which refer to the positive and negative electricities:—

$$\left.\begin{aligned}\frac{dx'}{dt}&=\frac{\partial x'}{\partial t}+c'\frac{\partial x'}{\partial s'},\\ \frac{dx'}{dt}&=\frac{\partial x'}{\partial t}-c'_1\frac{\partial x'}{\partial s'}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In a contingent second differentiation with respect to t, we should have to take into account that the quantities c' and c'_1 are again to be treated as functions of t and s', because at a fixed point of the conductor the current-velocity can vary with the time if the intensity of the current is variable, and also because at a fixed time the current-velocity can be different at different points of the conductor if the conductor has not everywhere an equal cross section and like quality.

The distance r between the particle of current-electricity in the conductor s' and the unit of electricity in the point x, y, z is likewise to be regarded as a function of t and s'; and the complete differential coefficients of r with respect to t are therefore to be formed in the following manner for the positive

and negative current-electricites:—

$$\left.\begin{aligned} \frac{dr}{dt} &= \frac{\partial r}{\partial t} + c'\frac{\partial r}{ds'}, \\ \frac{dr}{dt} &= \frac{\partial r}{\partial t} - c'_1\frac{\partial r}{\partial s'}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In these the partial differential coefficient $\frac{\partial r}{\partial t}$ comprises the two changes undergone by r, on the one hand through the motion of the unit of electricity, and on the other through the motion of the element ds' of the conductor containing the particle of electricity; while $\frac{\partial r}{\partial s'}$ refers to the change produced in r by the current-motion of the electricity-particle which takes place in the conductor.

Employing this method of notation, the x components of the force which a current-element ds' exerts upon the moved unit of electricity may now be determined, first, according to the fundamental electrodynamic law advanced by me, because it is the most convenient for the working and furnishes the simplest expressions, to which, in order to obtain the expressions corresponding to the two other fundamental laws, certain terms must then be added.

§ 3.

According to my fundamental law the x component of the force which one moved particle e of electricity undergoes from another, e', is represented by the formula

$$ee'\left\{\frac{\partial\frac{1}{r}}{\partial x}\left[-1+k\left(\frac{dx}{dt}\frac{dx'}{dt}+\frac{dy}{dt}\frac{dy'}{dt}+\frac{dz}{dt}\frac{dz'}{dt}\right)\right]-k\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)\right\},$$

which, if we signify a sum of three terms alike in form, which refer to the three coordinate-directions, by writing only the term referring to the x direction and prefixing to it the symbol of summation, can be written somewhat shorter thus—

$$ee'\left[\frac{\partial\frac{1}{r}}{\partial x}\left(-1+k\Sigma\frac{dx}{dt}\frac{dx'}{dt}\right)-k\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)\right].$$

We now take, in the point x', y', z', a current-element ds' in which the quantity of positive electricity $h'ds'$ flows with the velocity c', and the negative $-h'ds'$ with the velocity $-c'_1$; and we will first determine the x components of that force which the positive electricity $h'ds'$ exerts upon the above-

mentioned moved unit of electricity in the point x, y, z. For that purpose we have to substitute in the preceding expression 1 and $h'ds$ for e and e', by which we get

$$ds'\left[h'\frac{\partial\frac{1}{r}}{\partial x}\left(-1+k\Sigma\frac{dx}{dt}\frac{dx'}{dt}\right)-kh'\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)\right].$$

The product

$$h'\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)$$

we can bring into another form; and as the corresponding transformation has frequently to be employed in other cases also, we will presently work it out somewhat more generally. Let F be any quantity which, in the way stated of those referring to the positive current-electricity in the preceding section, depends on t and s'; then, in accordance with (1) and (2), we can write

$$\frac{d\mathrm{F}}{dt}=\frac{\partial\mathrm{F}}{\partial t}+c'\frac{\partial\mathrm{F}}{\partial s};$$

or, after multiplication by h',

$$h'\frac{d\mathrm{F}}{dt}=h'\frac{\partial\mathrm{F}}{\partial t}+h'c'\frac{\partial\mathrm{F}}{\partial s};$$

and this can be transformed into

$$h'\frac{d\mathrm{F}}{dt}=h'\frac{\partial\mathrm{F}}{\partial t}+\frac{\partial(h'c'\mathrm{F})}{\partial s'}-\mathrm{F}\frac{\partial(h'c')}{\partial s'}.$$

Herein another differential coefficient can be substituted for $\frac{\partial(h'c')}{\partial s'}$. The element ds' of the conductor is bounded by two cross sections corresponding to the lengths of arc s' and s' ds'. The two quantities of electricity which flow through these cross sections during the time dt, and of which the first passes into and the other passes out of the element ds', are represented by

$$h'c'dt \text{ and } \left(h'c'+\frac{\partial(h'c')}{\partial s'}ds'\right)dt;$$

and hence follows that the increase which takes place during the time dt, of the quantity of positive electricity in ds', is represented by

$$-\frac{\partial(h'c')}{\partial s'}ds'\,dt.$$

But the same increase can, on the other hand, be also denoted by

$$\frac{\partial h'}{\partial t}ds'\,dt;$$

and we consequently obtain the equation

$$\frac{\partial h'}{\partial t}=-\frac{\partial(h'c')}{\partial s'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Thereby the above equation is changed into

$$h'\frac{d\mathrm{F}}{dt}=h'\frac{\partial \mathrm{F}}{\partial t}+\frac{\partial(h'c'\mathrm{F})}{\partial s'}+\mathrm{F}\frac{\partial h'}{\partial t},$$

or, after contracting the first and last terms on the right-hand side, into

$$h'\frac{d\mathrm{F}}{dt}=\frac{\partial(h'\mathrm{F})}{\partial t}+\frac{\partial(h'c'\mathrm{F})}{\partial s'}. \quad . \quad . \quad . \quad . \quad (4)$$

Returning now to the expression of the x component of the force exerted by the quantity $h'ds'$ of positive electricity upon the unit of electricity, and applying the preceding method of transformation to the term $h'\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)$, in which $\frac{1}{r}\frac{dx'}{dt}$ is the quantity that was previously denoted generally by F, the expression changes into

$$ds'\left[h'\frac{\partial\frac{1}{r}}{\partial x}\left(-1+k\Sigma\frac{dx}{dt}\frac{dx'}{dt}\right)-k\frac{\partial}{\partial t}\left(\frac{h'}{r}\frac{dx'}{dt}\right)-k\frac{\partial}{\partial s'}\left(\frac{h'c'}{r}\frac{dx'}{dt}\right)\right].$$

The differential coefficient $\frac{dx'}{dt}$ in this may, lastly, pursuant to (1) be resolved into its two parts; the expression then takes the form

$$ds'\left\{h'\frac{\partial\frac{1}{r}}{\partial x}\left[-1+k\Sigma\frac{dx}{dt}\left(\frac{\partial x'}{\partial t}+c'\frac{\partial x'}{\partial s'}\right)\right]-k\frac{\partial}{\partial t}\left(\frac{h'}{r}\frac{\partial x'}{\partial t}+\frac{h'c'}{r}\frac{\partial x}{\partial s}\right.\right.$$
$$\left.\left.-k\frac{\partial}{\partial s'}\left(\frac{h'c'}{r}\frac{\partial x'}{\partial t}+\frac{h'c'^2}{r}\frac{\partial x'}{\partial s'}\right)\right\}.\right.$$

We can now express in a corresponding manner also the x components of that force which the quantity $-h'ds'$ of negative electricity (whose current-velocity is $-c'_1$) contained in the element ds' exerts upon the unit of electricity. To this end we have to substitute $-h'$ for h', and $-c'_1$ for c', in the

preceding expression, by which we get

$$ds'\left\{-h'\frac{\partial\frac{1}{r}}{\partial x}\left[-1+k\Sigma\frac{dx}{dt}\left(\frac{\partial x'}{\partial t}-c'_1\frac{\partial x'}{\partial s'}\right)\right]\right.$$
$$\left.-k\frac{\partial}{\partial t}\left(-\frac{h'}{r}\frac{\partial x'}{\partial t}+\frac{h'c'_1}{r}\frac{\partial x'}{\partial s'}\right)-k\frac{\partial}{\partial s'}\left(\frac{h'c'_1}{r}\frac{\partial x'}{\partial t}-\frac{h'c'^2_1}{r}\frac{\partial x'}{\partial s'}\right)\right\}.$$

The sum of these two expressions *represents the* x *component of the total force exerted by the current-element* ds′ *upon the unit of electricity.* On forming that sum several terms cancel one another, and others admit of simplification from the fact that for the product $h'(c'+c'_1)$ the symbol i', which signifies the intensity of the current in ds', can be substituted, whence it at the same time follows that the product $h'(c'^2-c'^2_1)$, which can also be written in the form $h'(c'+c'_1)(c'-c'_1)$, can be replaced by $i'(c'-c'_1)$. Hence, if we denote by $\mathfrak{x}ds'$ the x component of the force which the current-element ds' exerts upon the unit of electricity, we get the equation

$$\mathfrak{x}=k\left[i'\frac{\partial\frac{1}{r}}{\partial x}\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}-\frac{\partial}{\partial t}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)-\frac{\partial}{\partial s'}\left(\frac{i'}{r}\frac{\partial x'}{\partial t}+\frac{i'(c'-c'_1)}{r}\frac{\partial x'}{\partial s'}\right)\right]. \quad (5)$$

§ 4.

Riemann's fundamental law may now be treated in the same manner; and this is very easy in connexion with the foregoing.

The x component of the force which a moved electricity-particle e suffers from a moved electricity-particle e' is expressed, according to Riemann, by the formula

$$ee'\left\{\frac{\partial\frac{1}{r}}{\partial x}\left[-1-\frac{k}{2}\Sigma\left(\frac{dx}{dt}-\frac{dx'}{dt}\right)^2\right]+k\frac{d}{dt}\left[\frac{1}{r}\left(\frac{dx}{dt}-\frac{dx'}{dt}\right)\right]\right\}.$$

This formula can also be written as follows—

$$ee'\left[\frac{\partial\frac{1}{r}}{\partial x}\left(-1+k\Sigma\frac{dx}{dt}\frac{dx'}{dt}\right)-k\frac{d}{dt}\left(\frac{1}{r}\frac{dx'}{dt}\right)\right]$$
$$+ee'k\left\{-\frac{1}{2}\frac{\partial\frac{1}{r}}{\partial x}\Sigma\left[\left(\frac{dx}{dt}\right)^2+\left(\frac{dx'}{dt}\right)^2\right]+\frac{d}{dt}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\}.$$

The first term of this expression agrees perfectly with the ex-

pression which according to my fundamental law represents the force-component in question; we can therefore use for this term the developments already carried out in the preceding section, and need only carry out the developments for the second term.

To determine the force exerted by a current-element ds' upon a moved unit of electricity, let us consider in the element first, again, the positive electricity $h'ds'$, which flows with the velocity c'. In order to express for this electricity the portion of the force-component which corresponds to the second term of the preceding expression, we have to substitute in it 1 and $h'ds'$ for e and e', whereby we obtain

$$kds'\left\{-\frac{h'}{2}\frac{\partial\frac{1}{r}}{\partial x}\Sigma\left[\left(\frac{dx}{dt}\right)^2+\left(\frac{dx'}{dt}\right)^2\right]+h'\frac{d}{dt}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\}.$$

In this we put, in accordance with (1) and (2),

$$\frac{dx'}{dt}=\frac{\partial x'}{\partial t}+c'\frac{\partial x'}{\partial s'},$$

$$\frac{d}{dt}\left(\frac{1}{r}\frac{dx}{dt}\right)=\frac{\partial}{\partial t}\left(\frac{1}{r}\frac{dx}{dt}\right)+c'\frac{\partial}{\partial s'}\left(\frac{1}{r}\frac{dx}{dt}\right),$$

by which the expression is changed into

$$kds'\left\{-\frac{h'}{2}\frac{\partial\frac{1}{r}}{\partial x}\Sigma\left[\left(\frac{dx}{dt}\right)^2+\left(\frac{\partial x'}{\partial t}\right)^2+2c'\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+c'^2\left(\frac{\partial x'}{\partial s'}\right)^2\right]\right.$$
$$\left.+h'\frac{\partial}{\partial t}\left(\frac{1}{r}\frac{dx}{dt}\right)+h'c'\frac{\partial}{\partial s'}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\}.$$

The corresponding expression for the negative electricity $-h'ds'$, which flows with the velocity $-c'_1$, is

$$kds'\left\{\frac{h'}{2}\frac{\partial\frac{1}{r}}{\partial x}\Sigma\left[\left(\frac{dx}{dt}\right)^2+\left(\frac{\partial x'}{\partial t}\right)^2-2c'_1\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+c'^2_1\left(\frac{\partial x'}{\partial s'}\right)^2\right]\right.$$
$$\left.-h'\frac{\partial}{\partial t}\left(\frac{1}{r}\frac{dx}{dt}\right)+h'c'_1\frac{\partial}{\partial s'}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\}.$$

By addition of these two expressions we get

$$kds'\left\{-i'\frac{\partial\frac{1}{r}}{dx}\Sigma\left[\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\left(\frac{\partial x'}{\partial s'}\right)^2\right]+i'\frac{\partial}{\partial s'}\left(\frac{1}{r}\frac{dx}{dt}\right)\right\},$$

for which, on account of the self-evident equation

$$\Sigma\left(\frac{\partial x'}{\partial s'}\right)^2=\left(\frac{\partial x'}{\partial s'}\right)^2+\left(\frac{\partial y'}{\partial s'}\right)^2+\left(\frac{\partial z'}{\partial s'}\right)^2=1,$$

and because i' is independent of s' and hence in the last term can be put with the rest under the differentiation-symbol, can be also written

$$kds'\left[-i'\frac{\partial\frac{1}{r}}{\partial x}\left(\Sigma\frac{\partial x'}{\partial t'}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\right)+\frac{\partial}{\partial s'}\left(\frac{i'}{r}\frac{dx}{dt}\right)\right].$$

This is the constituent resulting from the *second* term of the above expression of the x component, of the force which the current-element ds' exerts upon a moved unit of electricity according to Riemann's fundamental law. The constituent resulting from the *first* term agrees, as already stated, with the value that holds good according to my fundamental law, of the force-component which we have denoted by $\mathfrak{x}ds'$ and determined in the preceding section. Hence, if we denote the total value of the force-component according to Riemann's fundamental law by $\mathfrak{x}_1ds'$, we get

$$\mathfrak{x}_1=\mathfrak{x}+k\left[-i'\frac{\partial\frac{1}{r}}{\partial x}\left(\Sigma\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\right)+\frac{\partial}{\partial s'}\left(\frac{i'}{r}\frac{dx}{dt}\right)\right].\quad (6)$$

§ 5.

Now, thirdly, Weber's fundamental law must be treated in the same manner.

According to this law, between two moved particles of electricity e and e' a repulsion takes place the intensity of which is

$$\frac{ee'}{r^2}\left[1-\frac{k}{2}\left(\frac{dr}{dt}\right)^2+kr\frac{d^2r}{dt^2}\right]$$

and from this, by multiplication with $\frac{x-x'}{r}$, we obtain the x component of the force which the particle e suffers, thus—

$$ee'\frac{x-x'}{r^3}\left[1-\frac{k}{2}\left(\frac{dr}{dt}\right)^2+kr\frac{d^2r}{dt^2}\right].$$

In applying this expression to the quantity of electricity $h'ds'$ flowing in the current-element ds' with the velocity c', and to the moved unit of electricity, we have again first to replace e and e' by 1 and $h'ds'$. We will then, in accordance

with (4) make the following transformation,

$$h'\frac{d^2r}{dt^2}=h'\frac{d}{dt}\left(\frac{dr}{dt}\right)=\frac{\partial}{\partial t}\left(h'\frac{dr}{dt}\right)+\frac{\partial}{\partial s'}\left(h'c'\frac{dr}{dt}\right),$$

and, besides, put everywhere

$$\frac{dr}{dt}=\frac{\partial r}{\partial t}+c'\frac{\partial r}{\partial s'}.$$

Then we get

$$ds'\frac{x-x'}{r^3}\left\{h'-\frac{k}{2}\left[h'\left(\frac{\partial r}{\partial t}\right)^2+2h'c'\frac{\partial r}{dt}\frac{\partial r}{\partial s'}+h'c'^2\left(\frac{\partial r}{\partial s'}\right)^2\right]\right.$$
$$\left.+kr\frac{\partial}{\partial t}\left(h'\frac{\partial r}{\partial t}+h'c'\frac{\partial r}{\partial s'}\right)+kr\frac{\partial}{\partial s'}\left(h'c'\frac{\partial r}{\partial t}+h'c'^2\frac{\partial r}{\partial s'}\right)\right\}.$$

Just so we obtain for the negative electricity $-h'ds'$, flowing with the velocity $-c'_1$,

$$ds'\frac{x-x'}{r^3}\left\{-h'-\frac{k}{2}\left[-h'\left(\frac{\partial r}{\partial t}\right)^2+2h'c'_1\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}-h'c'^2_1\left(\frac{\partial r}{\partial s'}\right)^2\right]\right.$$
$$\left.+kr\frac{\partial}{\partial t}\left(-h'\frac{\partial r}{\partial t}+h'c'_1\frac{\partial r}{\partial s'}\right)+kr\frac{\partial}{\partial s'}\left(h'c'_1\frac{\partial r}{\partial t}-h'c'^2_1\frac{\partial r}{\partial s'}\right)\right\}.$$

The sum of these two expressions represents the x component of the force which the entire current-element ds' must, according to Weber's fundamental law, exert upon the unit of electricity. If this is denoted by $\mathfrak{x}_2 ds'$, then we have

$$\mathfrak{x}_2=k\frac{x-x'}{r^3}\left\{-i'\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}+r\frac{\partial}{\partial t}\left(i'\frac{\partial r}{\partial s'}\right)-\frac{1}{2}i'(c'-c'_1)\left(\frac{\partial r}{\partial s'}\right)^2\right.$$
$$\left.+r\frac{\partial}{\partial s'}\left[i'\frac{\partial r}{\partial t}+i'(c'-c'_1)\frac{\partial r}{\partial s'}\right]\right\}. \quad (7)$$

This expression of $\mathfrak{x}_2$ can, like the above expression of $\mathfrak{x}_1$, be brought into such a form as to appear as the sum of $\mathfrak{x}$ and some superadded terms. For that purpose we will divide the preceding equation by k, then carry out on the right-hand side the suggested multiplication by $\frac{x-x'}{r^3}$, and at the same time resolve some of the terms. Above the resulting terms we will place numbers, in order to be able afterwards to designate them simply by the numbers:—

$$\frac{\mathfrak{x}_2}{k}=\overset{1}{-i'\frac{x-x'}{r^3}\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}}+\overset{2}{\frac{x-x'}{r^2}\frac{\partial}{\partial t}\left(i'\frac{\partial r}{\partial s'}\right)}-\overset{3}{\frac{i'(c'-c'_1)}{2}\frac{x-x'}{r^3}\left(\frac{\partial r}{\partial s'}\right)^2}$$
$$+\overset{4}{i'\frac{x-x'}{r^2}\frac{\partial^2 r}{\partial t\partial s'}}+\overset{5}{\frac{x-x'}{r^2}\frac{\partial}{\partial s'}\left[i'(c'-c'_1)\frac{\partial r}{\partial s'}\right]}.$$

In a similar manner we will treat the expression of $\mathfrak{x}$ given in equation (5); but at the same time we will besides transform the first term separately. We can, namely, put

$$\frac{\partial^2(r^2)}{\partial t\,\partial s'}=2\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}+2r\frac{\partial^2 r}{\partial t\,\partial s'};$$

and simultaneously we get from $r^2=\Sigma(x-x')^2$

$$\frac{\partial^2(r^2)}{\partial t\,\partial s'}=-2\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}-2\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}.$$

From the combination of these two equations results

$$\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}=-\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}-r\frac{\partial^2 r}{\partial t\,\partial s'}-\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}.$$

The algebraic sum standing here on the right-hand side we will insert in equation (5) for $\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}$. At the same time we will reverse all the signs of this equation, so that, after division by k, it will determine the quantity $-\frac{\mathfrak{x}}{k}$, in the following manner:—

$$-\frac{\mathfrak{x}}{k}=\overset{6}{-i'\frac{x-x'}{r^3}\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}}\overset{7}{-i'\frac{x-x'}{r^2}\frac{\partial^2 r}{\partial t\,\partial s'}}\overset{8}{-i'\frac{x-x'}{r^3}\frac{\partial}{\partial s'}\Sigma(x-x')\frac{dx'}{dt}}$$

$$\overset{9}{+\frac{\partial}{\partial t}\left(\frac{i'}{r}\right)\frac{\partial x'}{\partial s'}}\overset{10}{+\frac{i'}{r}\frac{\partial^2 x'}{\partial t\,\partial s'}}\overset{11}{+\frac{\partial}{\partial s'}\left(\frac{i'}{r}\frac{\partial x'}{\partial t}\right)}\overset{12}{+\frac{\partial}{\partial s'}\left(\frac{i'(c'-c'_1)}{r}\frac{\partial x'}{\partial s'}\right)}.$$

The twelve terms occurring in these two expressions form together the expression of $\frac{\mathfrak{x}_2-\mathfrak{x}}{k}$; and now it requires to be brought into a form as simple as possible and suitable for the further calculations; this can be done by suitable grouping of the terms. We obtain, namely, if we indicate the terms briefly by their numbers:—

$$4+7=0;$$

$$1+6+2+9=-\frac{\partial}{\partial s'}\left[(x-x')\frac{\partial}{\partial t}\left(\frac{i'}{r}\right)\right];$$

$$8+10=\frac{\partial}{\partial x}\left[\frac{i'}{r}\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}\right];$$

$$3+5+12=-\frac{\partial}{\partial x}\left[\frac{i'(c'-c'_1)}{2r}\left(\frac{\partial r}{\partial s'}\right)^2\right]-\frac{\partial}{\partial s'}\left[i'(c'-c'_1)\frac{\partial^2 r}{\partial x\,\partial s'}\right].$$

Herewith all the terms except the 11th, which will have to

be taken into consideration separately, are brought into calculation; and hence we get, on the whole,

$$\frac{\mathfrak{x}_2-\mathfrak{x}}{k}=\frac{\partial}{\partial x}\left[\frac{i'}{r}\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}\right]-\frac{\partial}{\partial x}\left[\frac{i'(c'-c'_1)}{2r}\left(\frac{\partial r}{\partial s'}\right)^2\right]$$
$$-\frac{\partial}{\partial s'}\left[(x-x')\frac{\partial}{\partial t}\left(\frac{i'}{r}\right)\right]+\frac{\partial}{\partial s'}\left(\frac{i'}{r}\frac{\partial x'}{\partial t}\right)-\frac{\partial}{\partial s'}\left[i'(c'-c'_1)\frac{\partial^2 r}{\partial x\partial s'}\right];$$

and since all the terms herein are differential coefficients with respect to x or s', they can be collected into two differential coefficients. From this equation we get the sought expression of $\mathfrak{x}_2$, namely

$$\mathfrak{x}_2=\mathfrak{x}+k\frac{\partial}{\partial x}\left[\frac{i'}{r}\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{i'(c'-c'_1)}{2r}\left(\frac{\partial r}{\partial s'}\right)^2\right]$$
$$-k\frac{\partial}{\partial s'}\left[(x-x')\frac{\partial}{\partial t}\left(\frac{i'}{r}\right)-\frac{i'}{r}\frac{\partial x'}{\partial t}+i'(c'-c'_1)\frac{\partial^2 r}{\partial x\, ds'}\right]. \quad (8)$$

§ 6.

In the three preceding sections the x component of the force exerted by a current-element ds' upon a moved unit of electricity is deduced from the three fundamental laws. In each of the three expressions (5), (6), and (8) there is a term which is a differential coefficient with respect to s', and which therefore vanishes in the integration over a *closed* current s'. Hence the force exerted by a closed current, or even by a system of closed currents, is represented by expressions of simplified form, which we will now consider more closely.

We start from the expression given in equation (5). When we multiply this by ds' and then integrate it over a closed current or a system of closed currents, we obtain the x component of that force which, according to my fundamental law, the current, or system of currents, must exert on a moved unit of electricity. These components being denoted by $\mathfrak{X}$, we get

$$\mathfrak{X}=k\int i'\frac{\partial\frac{1}{r}}{\partial x}\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}ds'-k\int\frac{\partial}{\partial t}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)ds'. \quad . \quad . \quad (9)$$

In this equation it is tacitly presupposed that the length of the closed conductor s' remains unchanged, so that those elements ds' which at a given time form the closed conductor form it also during the succeeding time, and no element enters or leaves it. In reality, however, cases may occur in which the length of the conductor changes—for instance, when at a place a sliding of two parts of the conductor on one another

takes place, causing parts of the conductor which were previously outside of the circuit to be afterwards within it, or *vice versâ.* In the parts which in this process are added the current commences; in those withdrawn it ceases; and by this alteration of the current-intensity in particular parts of the conductor a force is conditioned, which must also be taken into account. It is true that, on account of the great velocity with which the commencing and cessation of the current are accomplished, the parts of the conductor in which at any moment they take place are very small; but the differential coefficient $\frac{\partial i'}{\partial t}$ for them is very great, and through this the corresponding portion of the force may take a considerable value. The question now is, how this part of the force can be also expressed in the formula.

We will choose the places where the entry and exit of parts of the conductor take place as the initial and final points respectively of the closed conductor s', so that a newly entering piece of conductor is annexed exactly at the end of the conductor. If s'_1 denote the length of the conductor at the time t, the element of conductor added during the time-element will be represented by $\frac{ds'_1}{dt}dt$. If, further, the very short time requisite for the production of the current in a conductor-piece entering the closed circuit be denoted by τ, then will, during the lengthening of the conductor, a piece at the end of it of the length $\frac{ds'_1}{dt}\tau$ be that in which the origination of the current takes place. This origination is an increase, taking place during the time τ, from 0 to the value i' prevailing for the rest of the conduction. The mean value of the differential coefficient $\frac{\partial i}{\partial t}$ in this piece during the time τ is consequently $=\frac{i'}{\tau}$; and so we can represent the corresponding mean value of the differential coefficient $\frac{\partial}{\partial t}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)$ by $\frac{1}{\tau}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)_1$, wherein the index 1 put to the brackets is to indicate that the quantities r and $\frac{\partial x'}{\partial s'}$ within the brackets have the values belonging to s'_1.

Now, in order to bring likewise into calculation in our formula the origination of the current in this small piece of conductor, we have to add to the second integral in the formula, which if we write it with the limits has the form

$$\int_0^{s'_1}\frac{\partial}{\partial t}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)ds',$$

a quantity which is the product of the just-determined mean differential coefficient and the length of the piece of conductor in question; thus

$$\frac{1}{\tau}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)_1\frac{ds'_1}{dt}\tau=\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)_1\frac{ds'_1}{dt}.$$

Consequently we have to put in the place of the preceding integral the following sum—

$$\int_0^{s'_1}\frac{\partial}{\partial t}\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)ds'+\left(\frac{i'}{r}\frac{\partial x'}{\partial s'}\right)_1\frac{ds'_1}{dt}.$$

But this sum is nothing else but the differential coefficient, taken with respect to t, of the integral

$$\int_0^{s'_1}\frac{i'}{r}\frac{\partial x'}{\partial s'}ds',$$

if therein not only the quantity under the integral-symbols but also the upper limit s'_1 be regarded as a function of t. The alteration to be undertaken with the above integral consists therefore only in this, that the differentiation there indicated under the integral-symbol is to be indicated before it. Moreover it must be remarked that the integral extended over the whole of the closed circuit is not, like one referred to a single conductor-element, to be looked on as a function of t and s', but as a function of t only, and that hence, in indicating the differentiation, d can be employed in this case instead of ∂, so that the expression will be

$$\frac{d}{dt}\int_0^{s'_1}\frac{i'}{r}\frac{\partial x'}{\partial s'}ds'.$$

Accordingly equation (9), when account is taken of the circumstance that the length of the conductor can change, changes into the following, in which we will now, for simplicity, omit the limits of the integral (the adding of which was expedient for the preceding consideration), because, after it has once been said that all the integrals are to be extended over the entire closed conductor, they are understood:—

$$\mathfrak{X}=k\frac{\partial}{\partial x}\int\frac{i'}{r}\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}ds'-k\frac{d}{dt}\int\frac{i'}{r}\frac{\partial x'}{\partial s'}ds'. \quad . \quad . \quad (10)$$

In the same way, denoting by $\mathfrak{X}_1$ and $\mathfrak{X}_2$ those values which the same force-component must take according to Riemann's and Weber's fundamental laws, we obtain from equations (6)

and (8) the following equations:—

$$\mathfrak{X}_1=\mathfrak{X}-k\frac{\partial}{\partial x}\int\frac{i'}{r}\left(\Sigma\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\right)ds'; \quad . \quad . \quad . \quad (11)$$

$$\mathfrak{X}_2=\mathfrak{X}+k\frac{\partial}{\partial x}\int\frac{i'}{r}\left[\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{c'-c'_1}{2}\left(\frac{\partial r}{\partial s'}\right)^2\right]ds'. (12)$$

Precisely corresponding expressions to those here derived for the x component of the force of course holds good also for y and z components.

§ 7.

The three force-components referring to the three directions of coordinates can now, in the manner discussed in § 1, be traced back to one quantity, from which they can be derived by differentiation. This is the electrodynamic potential of the closed current or system of currents upon the moved unit of electricity existing in the point x, y, z. Now, as with the forces which are independent of the motion that potential of a given agent which has reference to *a unit* of the same agent *supposed to be concentrated in a point* is by Green named the *potential function*, we will here also introduce the same distinction, and call the electrodynamic potential of a closed current or current-system, *so far as it refers to a unit of electricity supposed concentrated in a point*, the electrodynamic *potential function*.

This electrodynamic potential function is distinguished (as was mentioned in § 1), even externally, from Green's potential function, which refers to forces that are independent of the motion. It contains, namely, not merely the coordinates x, y, z of the unit of electricity, but also their velocity-components $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$. Further, the operation by means of which the force-components are to be derived from the electrodynamic potential function is the same operation as that to which, according to Lagrange, the *vis viva*, expressed in universal coordinates, is to be submitted in the derivation of the components of the force. For if the electrodynamic potential function be denoted by Π, and the x component of the force by $\mathfrak{X}$, then the following equation can be formed:—

$$\mathfrak{X}=\frac{\partial\Pi}{\partial x}-\frac{d}{dt}\left(\frac{\partial\Pi}{\partial\frac{dx}{dt}}\right). \quad . \quad . \quad . \quad . \quad (13)$$

We have now to construct the forms of the potential func-

tion of a closed current corresponding to the three fundamental laws.

According to my fundamental law the electrodynamic potential of two quantities e and e' of electricity, supposed to be concentrated in points, is represented by

$$k\frac{ee'}{r}\Sigma\frac{dx}{dt}\frac{dx'}{dt}.$$

If in employing this formula we put for e the unit of electricity, and for e' successively the two quantities $h'ds'$ and $-h'ds'$ of electricity contained in a current-element ds', and in regard to the velocity-components of the latter take into account that they flow in the conductor in opposite directions with the velocities c' and c'_1, while they have in common any motion of the conductor, and if we then form the sum of these two expressions, putting $h'(c'+c'_1)=i'$, and, lastly, integrate this sum over the closed current, we get

$$\Pi=k\int\frac{i'}{r}\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}ds'. \quad . \quad . \quad . \quad . \quad (14)$$

If this expression of Π be now inserted in equation (13), we obtain from it in fact the value for $\mathfrak{X}$ determined by equation (10).

Since the velocity-components $\frac{dx}{dt}$, $\frac{dy}{dt}$, and $\frac{dz}{dt}$, occurring in the expression of Π, are independent of the quantity s', with respect to which the integration is to be performed, we can put them outside of the integral-symbol and then give to the expression the following form:—

$$\Pi=k\Sigma\frac{dx}{dt}\int\frac{i'}{r}\frac{\partial x'}{\partial s'}ds'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

The sum here indicated contains three integrals, which differ from each other only by this—that in them either $\frac{\partial x'}{\partial s'}$, or $\frac{\partial y'}{\partial s'}$, or $\frac{\partial z'}{ds'}$ occurs. These three integrals, together with the factor k, we will, for brevity, represent by simple symbols, putting

$$\mathrm{H}_x=k\int\frac{i'}{r}\frac{\partial x'}{\partial s'}ds', \quad \mathrm{H}_y=k\int\frac{i'}{r}\frac{\partial y'}{\partial s'}ds', \quad \mathrm{H}_z=k\int\frac{i'}{r}\frac{\partial z'}{\partial s'}ds'. \, (16)$$

Then we get

$$\Pi=\mathrm{H}_x\frac{dx}{dt}+\mathrm{H}\ \frac{dy}{dt}+\mathrm{H}_z\frac{dz}{dt}, \quad . \quad . \quad . \quad (17)$$

or, the sign of summation being employed,

$$\Pi = \Sigma \mathrm{H}_x \frac{dx}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17\text{ A})$$

This changes equation (13) into

$$\mathfrak{X} = \frac{\partial \mathrm{H}_x}{\partial x}\frac{dx}{dt} + \frac{\partial \mathrm{H}_y}{\partial x}\frac{dy}{dt} + \frac{\partial \mathrm{H}^z}{\partial x}\frac{dz}{dt} - \frac{d\mathrm{H}_x}{dt}, \quad . \quad (18)$$

or, with the aid of the symbol of summation,

$$\mathfrak{X} = \frac{\partial}{\partial x}\Sigma \mathrm{H}_x \frac{dx}{dt} - \frac{d\mathrm{H}_x}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18\text{ A})$$

According to the fundamental laws of Riemann and Weber, the electrodynamic potential of two moved quantities of electricity e and e', supposed concentrated in points, upon each other is represented by the expressions

$$-\frac{k}{2}\frac{ee'}{r}\Sigma\left(\frac{dx}{dt} - \frac{dx'}{dt}\right)^2,$$

$$-\frac{k}{2}\frac{ee'}{r}\left(\frac{dr}{dt}\right)^2.$$

From these are obtained for the potential of a closed current s' upon a unit of electricity, consequently for the *potential function* of the closed current, which according to these laws may be denoted by Π_1 and Π_2, the expressions :—

$$\Pi_1 = k\int \frac{i'}{r}\left[\Sigma\left(\frac{dx}{dt} - \frac{\partial x'}{\partial t}\right)\frac{\partial x'}{\partial s'} - \frac{c' - c'_1}{2}\right] ds'; \quad . \quad (19)$$

$$\Pi_1 = -k\int \frac{i'}{r}\left[\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'} + \frac{c' - c'_1}{2}\left(\frac{\partial r}{\partial s'}\right)^2\right] ds'. \quad . \quad . \quad (20)$$

The latter expression can be transformed in the following manner. From

$$r^2 = \Sigma(x - x')^2$$

is obtained

$$r\frac{\partial r}{dt} = \Sigma(x - x')\left(\frac{dx}{dt} - \frac{\partial x'}{\partial t}\right)$$

$$= \Sigma(x - x')\frac{dx}{dt} - \Sigma(x - x')\frac{\partial x'}{\partial t};$$

and from this we get, further, by differentiation with respect to s',

$$\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'} + r\frac{\partial^2 r}{\partial t\,\partial s'} = -\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'} - \frac{\partial}{\partial s'}\Sigma(x - x')\frac{\partial x'}{\partial t},$$

and consequently

$$\frac{1}{r}\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}=-\frac{1}{r}\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}-\frac{1}{r}\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{\partial^2 r}{\partial t\,\partial s'}.$$

Putting now, in equation (20), for $\frac{1}{r}\frac{\partial r}{\partial t}\frac{\partial r}{\partial s'}$, the expression here found, whose last term gives 0 in the integration, we obtain

$$\Pi_2=k\int\frac{i'}{r}\left[\Sigma\frac{dx}{dt}\frac{\partial x'}{\partial s'}+\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{c'-c'_1}{2}\left(\frac{\partial r}{\partial s'}\right)^2\right]ds'. \quad (21)$$

In the two expressions (19) and (21) of Π_1 and Π_2 the first term arising on the resolution of the brackets agrees with the expression of Π given under (14); hence we can write:

$$\Pi_1=\Pi-k\int\frac{i'}{r}\left(\Sigma\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\right)ds', \quad . \quad . \quad . \quad . \quad (22)$$

$$\Pi_2=\Pi+k\int\frac{i'}{r}\left[\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{c'-c'_1}{2}\left(\frac{\partial r}{\partial s'}\right)^2\right]ds'. \quad (23)$$

If we now form, corresponding with equations (13), the equations

$$\mathfrak{X}_1=\frac{\partial\Pi_1}{\partial x}-\frac{d}{dt}\left(\frac{\partial\Pi_1}{\partial\frac{dx}{dt}}\right), \quad . \quad . \quad . \quad . \quad . \quad (24)$$

$$\mathfrak{X}_2=\frac{\partial\Pi_2}{\partial x}-\frac{d}{dt}\left(\frac{\partial\Pi_2}{\partial\frac{dx}{dt}}\right), \quad . \quad . \quad . \quad . \quad . \quad (25)$$

and if, in these, for Π_1 and Π_2 we employ the previously given expressions, in which the terms added to Π do not contain the velocity-components $\frac{dx}{dt}$, $\frac{dy}{dt}$, and $\frac{dz}{dt}$, and hence give 0 on differentiation with respect to these quantities, we obtain for $\mathfrak{X}_1$ and $\mathfrak{X}_2$ the expressions given under (11) and (12).

For abbreviation, simple symbols may be brought in for those additional terms independent of $\frac{dx}{dt}$, $\frac{dy}{dt}$, and $\frac{dz}{dt}$, by putting

$$G_1=-k\int\frac{i'}{r}\Sigma\frac{\partial x'}{\partial t}\frac{\partial x'}{\partial s'}+\frac{c'-c'_1}{2}\Big)ds', \quad . \quad . \quad . \quad . \quad . \quad (26)$$

$$G_2=k\int\frac{i'}{r}\left[\frac{\partial}{\partial s'}\Sigma(x-x')\frac{\partial x'}{\partial t}-\frac{c'-c'_1}{2}\left(\frac{\partial r}{\partial s'}\right)^2\right]ds'. \quad . \quad (27)$$

The result is

$$\Pi_1 = \Pi + G_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

$$\Pi_2 = \Pi + G_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

whereby equations (24) and (25) are changed into the following,

$$\mathfrak{X}_1 = \frac{\partial(\Pi + G_1)}{\partial x} - \frac{d}{dt}\left(\frac{\partial \Pi}{\partial \frac{dx}{dt}}\right), \quad . \quad . \quad . \quad . \quad (30)$$

$$\mathfrak{X}_2 = \frac{\partial(\Pi + G_2)}{\partial x} - \frac{d}{dt}\left(\frac{\partial \Pi}{\partial \frac{dx}{dt}}\right), \quad . \quad . \quad . \quad . \quad (31)$$

which in conjunction with (13) are very convenient for comparison of the results of the three fundamental laws.

The electrodynamic potential function of a closed current (or system of currents) above introduced, and denoted, in its three forms corresponding to the three fundamental laws by Π, Π_1, and Π_2, is readily perceived to be very different from that potential function of which the differential coefficients occur already in Ampère's theory of the ponderomotive forces, and which in a previously published analysis* I named the *magnetic* potential function of the closed current, and denoted by P. This latter is obtained when, in the well-known manner, two magnetic surfaces are imagined to be substituted for the closed current, and then Green's potential function is formed for the quantities of magnetism present on those surfaces; accordingly its obvious signification is that it represents by its differential coefficients with respect to x, y, and z, taken negatively, the components which fall into the directions of the coordinates, of that force which the closed current exerts upon a unit of *magnetism* conceived as situated in the point x, y, z. It can only serve indirectly, and with the aid of special theoretical considerations, for the determination of the ponderomotive force exerted upon a *current-element* and of the electromotive force induced in it. The electrodynamic potential, on the contrary, which can be used directly for the determination of the force exerted upon a moved *unit of electricity*, needs only to be applied to the electricity in the conductor in order at once to determine the ponderomotive and the electromotive force.

* *Die mechanische Behandlung der Electricität,* Abschnitt VIII. p. 211.

§ 8.

Now, in order to deduce from the preceding formulæ the *ponderomotive* force exerted upon a current-element by a closed current, we first form from the potential function the potentials of the closed current upon the two electricities flowing in the current-element. From these we get, by the above-stated operation, the components, in any one direction (*e. g.* the x direction), of the forces which the two electricities undergo; and the sum of these two components is the respective force-component which refers to the entire current-element.

Suppose, then, given, in the point x, y, z, an element of current ds, in which the electricities hds and $-hds$ flow in opposite directions with the velocities c and c_1. Now, by my fundamental law we first employ for the potential function the value

$$\Pi=\Sigma \mathrm{H}_x \frac{dx}{dt},$$

given in equation (17 A), and obtain for the quantity hds of positive electricity:—

$$\text{Potential}=hds\,\Sigma\,\mathrm{H}_x \frac{dx}{dt},$$

$$\text{Force-component}=hds\left(\frac{\partial}{\partial x}\Sigma\,\mathrm{H}_x\frac{dx}{dt}-\frac{d\mathrm{H}_x}{dt}\right).$$

In these we must put

$$\frac{dx}{dt}=\frac{\partial x}{dt}+c\frac{\partial x}{\partial s},$$

$$\frac{d\mathrm{H}_x}{dt}=\frac{\partial \mathrm{H}_x}{\partial t}+c\frac{\partial \mathrm{H}_x}{\partial s},$$

by which the expressions are changed into:—

$$\text{Potential}=hds\Sigma\mathrm{H}_x\left(\frac{\partial x}{\partial t}+c\frac{\partial x}{\partial s}\right),$$

$$\text{Force-comp.}=hds\left[\frac{\partial}{\partial x}\Sigma\mathrm{H}_x\left(\frac{\partial x}{\partial t}+c\frac{\partial x}{\partial s}\right)-\frac{\partial \mathrm{H}_x}{\partial t}-c\frac{\partial \mathrm{H}_x}{\partial s}\right].$$

In precisely the same way we get for the quantity $-hds$ of negative electricity (for which we must bring into use the velocity of flow $-c_1$):—

$$\text{Potential}=-hds\Sigma\mathrm{H}_x\left(\frac{\partial x}{\partial t}-c_1\frac{\partial x}{\partial s}\right),$$

$$\text{Force-component}=-hds\frac{\partial}{\partial x}\Sigma\mathrm{H}_x\left(\frac{\partial x}{\partial t}-c_1\frac{\partial x}{\partial s}\right)-\frac{\partial \mathrm{H}_x}{\partial t}+c_1\frac{\partial \mathrm{H}_x}{\partial s}\Big].$$

If we now add the expressions referring to the two electricities, we get for the total current-element ds:—

$$\text{Potential} = hds(c+c_1)\Sigma \mathrm{H}_x \frac{\partial x}{\partial s},$$

$$\text{Force-component} = hds(c+c_1)\left(\frac{\partial}{\partial x}\Sigma \mathrm{H}_x \frac{\partial x}{\partial s} - \frac{\partial \mathrm{H}_x}{\partial s}\right),$$

or, if i denotes the product $h(c+c_1)$, which signifies the current-intensity in ds,

$$\text{Potential} = ids\Sigma \mathrm{H}_x \frac{\partial x}{\partial s},$$

$$\text{Force-component} = ids\left(\frac{\partial}{\partial x}\Sigma \mathrm{H}_x \frac{\partial x}{\partial s} - \frac{\partial \mathrm{H}_x}{\partial s}\right).$$

We will now denote the potential of the closed current upon the current-element ds by $\mathrm{U}ds$, and the x component of the force undergone by the current-element by Ξds; we have then, for the determination of U, taking also into account equations (16), to put

$$\mathrm{U} = i\Sigma \mathrm{H}_x \frac{\partial x}{\partial s} = ki\int \frac{i'}{r}\Sigma \frac{\partial x}{\partial s}\frac{\partial x'}{\partial s'}ds'; \quad . \quad . \quad . \quad (32)$$

and regarding this quantity U as a function of x, y, z, $\frac{\partial x}{\partial s}$, $\frac{\partial y}{\partial s}$, $\frac{\partial z}{\partial s}$, we can give to the expression of Ξ the following form:—

$$\Xi = \frac{\partial \mathrm{U}}{\partial x} - \frac{\partial}{\partial s}\left(\frac{\partial \mathrm{U}}{\partial \frac{\partial x}{\partial s}}\right). \quad . \quad . \quad . \quad . \quad (33)$$

If, instead of the potential function Π corresponding to my fundamental law, the potential-function $\Pi_1 = \Pi + \mathrm{G}_1$ or $\Pi_2 = \Pi + \mathrm{G}_2$, corresponding to the fundamental laws of Riemann or Weber, be employed, we have only to take also into account separately the additional term G_1 or G_2. But this, since it is independent of the velocity-components $\frac{dx}{dt}$, $\frac{dy}{dt}$, $\frac{dz}{dt}$, is equal for both the electricities flowing in ds, and hence, after multiplication by hds and $-hds$, is cancelled in the addition. Accordingly, in regard to the potential of a closed current upon a current-element, and in regard to the ponderomotive force exerted by a closed current upon an element of the current, there is no difference between the three laws; equations (32) and (33) hold in all three cases*.

* I will here incidentally remark that if we had been treating only of the ponderomotive force, and not at the same time of the electromotive

§ 9.

We now turn to the determination of the *electromotive* force which is induced in an element of the conductor by a closed current or system of currents.

For it we have only to determine the component of the force

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force also, the consideration of the subject could have been simplified. That is to say, we obtain for the ponderomotive force, even with single current-elements acting upon one another, expressions which contain not the velocities of the positive and the negative electricity as quantities to be separately dealt with, but only the current-intensity on the whole. According to my fundamental law the expressions for this case have the very same form as for the case in which the current exerting the force is closed. If the potential of the two current-elements ds and ds' upon each other is denoted by $u\,ds\,ds'$, and the x component of the force which ds suffers from ds' by $\xi ds\,ds'$, then we can put

$$u=k\frac{ii'}{r}\Sigma\frac{\partial x}{\partial s}\frac{\partial x'}{\partial s'},$$

$$\xi=\frac{\partial u}{\partial x}-\frac{\partial}{\partial s}\left(\frac{\partial u}{\partial\frac{\partial x}{\partial s}}\right).$$

According to Riemann's fundamental law the same expression holds good for the potential; but the operation to be employed for the derivation of the force-component is somewhat more complicated, namely

$$\xi_1=\frac{\partial u}{\partial x}-\frac{\partial}{\partial s}\left(\frac{\partial u}{\partial\frac{\partial x}{\partial s}}\right)+\frac{\partial}{\partial s'}\left(\frac{\partial u}{\partial\frac{\partial x'}{\partial s'}}\right).$$

Finally, according to Weber's fundamental law, for the potential, which in this case may be denoted by $u_2ds\,ds'$, the equation

$$u_2=-k\frac{ii'}{r}\frac{\partial r}{\partial s}\frac{\partial r}{\partial s'}=kii'\left(\frac{1}{r}\Sigma\frac{\partial x}{\partial s}\frac{\partial x'}{\partial s'}+\frac{\partial^2 r}{\partial s\,\partial s'}\right)$$

is valid; and for the derivation of the force-component the same operation as with Riemann's law is to be employed, namely

$$\xi_2=\frac{\partial u_2}{\partial x}-\frac{\partial}{\partial s}\left(\frac{\partial u_2}{\partial\frac{\partial x}{\partial s}}\right)+\frac{\partial}{\partial s'}\left(\frac{\partial u_2}{\partial\frac{\partial x'}{\partial s'}}\right).$$

According to this the ponderomotive force can be deduced from the potential of each two current-elements upon one another; but this potential, notwithstanding its partially correspondent form, is clearly to be distinguished from the quantity which is obtained when, of Neumann's potential of two closed currents upon one another, the part corresponding to two single current-elements ds and ds' is taken. For Neumann's potential is the *magnetic* potential, and consequently a potential of the same sort as Green's, while the thing in question here is the *electrodynamic* potential, on which account also an operation quite other than with Green's potential is requisite in order to derive the force-components.

exerted in the direction of the conductor-element by the current or system of currents upon a unit of electricity (to which we can ascribe any velocity of flow, c, we please) imagined in the conductor-element. The force-components falling into the directions of the coordinates are, according to our previous notation, to be represented by $\mathfrak{X}$, $\mathfrak{Y}$, and $\mathfrak{Z}$; and, in correspondence with this, we will denote by $\mathfrak{S}$ the force-component falling into the direction of the element ds, therefore into the s direction. We have then to put

$$\mathfrak{S}=\mathfrak{X}\frac{\partial x}{\partial s}-\mathfrak{Y}\frac{\partial y}{\partial s}+\mathfrak{Z}\frac{\partial z}{\partial s}=\Sigma\mathfrak{X}\frac{\partial x}{\partial s}. \quad . \quad . \quad (34)$$

In this we must now insert for the quantities $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$ their values resulting from the three fundamental laws.

According to my fundamental law, in accordance with (13) we can put

$$\mathfrak{X}=\frac{\partial \Pi}{\partial x}-\frac{d}{dt}\left(\frac{\partial \Pi}{\partial \frac{dx}{dt}}\right);$$

and consequently

$$\mathfrak{S}=\Sigma\frac{\partial \Pi}{\partial x}\frac{\partial x}{\partial s}-\Sigma\frac{\partial x}{\partial s}\frac{d}{dt}\left(\frac{\partial \Pi}{\partial \frac{dx}{dt}}\right).$$

If herein we use for Π the expression given under (17), namely

$$\Pi=\mathrm{H}_x\frac{dx}{dt}+\mathrm{H}_y\frac{dy}{dt}+\mathrm{H}_z\frac{dz}{dt},$$

we can put, if we wish to write all the terms singly,

$$\begin{aligned}\Sigma\frac{\partial \Pi}{\partial x}\frac{\partial x}{\partial s}=&\frac{dx}{dt}\left(\frac{\partial \mathrm{H}_x}{\partial x}\frac{\partial x}{\partial s}+\frac{\partial \mathrm{H}_x}{\partial y}\frac{\partial y}{\partial s}+\frac{\partial \mathrm{H}_x}{\partial z}\frac{\partial z}{\partial s}\right)\\ &+\frac{dy}{dt}\left(\frac{\partial \mathrm{H}_y}{\partial x}\frac{\partial x}{\partial s}+\frac{\partial \mathrm{H}_y}{\partial y}\frac{\partial y}{\partial s}+\frac{\partial \mathrm{H}_y}{\partial z}\frac{\partial z}{\partial s}\right)\\ &+\frac{dz}{dt}\left(\frac{\partial \mathrm{H}_z}{\partial x}\frac{\partial x}{\partial s}+\frac{\partial \mathrm{H}_z}{\partial y}\frac{\partial y}{\partial s}+\frac{\partial \mathrm{H}_z}{\partial z}\frac{\partial z}{\partial s}\right).\end{aligned}$$

Now, since the quantities H_x, H_y, and H_z depend on s only inasmuch as the coordinates occurring in them, x, y, z, of the unit of electricity, are dependent on s, the three sums in brackets represent the differential coefficients of the three quantities with respect to s; and hence we can write:—

$$\Sigma\frac{\partial \Pi}{\partial x}\frac{\partial x}{\partial s}=\frac{dx}{dt}\frac{\partial \mathrm{H}_x}{\partial s}+\frac{dy}{dt}\frac{\partial \mathrm{H}_y}{\partial s}+\frac{dz}{dt}\frac{\partial \mathrm{H}_z}{\partial s},$$

or, now bringing in again the symbol of summation on the right-hand side,

$$\Sigma \frac{\partial \Pi}{\partial x} \frac{\partial x}{\partial s} = \Sigma \frac{\partial \mathrm{H}_x}{\partial s} \frac{dx}{dt}.$$

Accordingly the above equation for $\mathfrak{S}$ passes into

$$\mathfrak{S} = \Sigma \frac{\partial \mathrm{H}_x}{\partial s} \frac{dx}{dt} - \Sigma \frac{\partial x}{\partial s} \frac{d\mathrm{H}_x}{dt}. \quad . \quad . \quad . \quad . \quad (35)$$

As, then, the unit of electricity has a double motion, namely the motion of the conductor-element and the flowing motion which takes place in the conductor-element with the velocity c, we will, in correspondence with the notation we have previously employed, put

$$\frac{dx}{dt} = \frac{\partial x}{\partial t} + c \frac{\partial x}{\partial s},$$

$$\frac{d\mathrm{H}_x}{dt} = \frac{\partial \mathrm{H}_x}{\partial t} + c \frac{\partial \mathrm{H}_x}{\partial s},$$

in which the differentiation indicated by $\frac{\partial}{\partial t}$ refers to the variations which are independent of the flowing motion of the unit of electricity. We thus obtain

$$\mathfrak{S} = \Sigma \frac{\partial \mathrm{H}_x}{\partial s} \left(\frac{\partial x}{\partial t} + c \frac{\partial x}{\partial s} \right) - \Sigma \frac{\partial x}{\partial s} \left(\frac{\partial \mathrm{H}_x}{\partial t} + c \frac{\partial \mathrm{H}_x}{\partial s} \right).$$

Here the terms containing the factor c cancel one another, and there remains

$$\mathfrak{S} = \Sigma \frac{\partial \mathrm{H}_x}{\partial s} \frac{\partial x}{\partial t} - \Sigma \frac{\partial \mathrm{H}_x}{\partial t} \frac{\partial x}{\partial s}. \quad . \quad . \quad . \quad . \quad (36)$$

To this expression of $\mathfrak{S}$ we can give a somewhat different form by adding the quantity

$$\Sigma \mathrm{H}_x \frac{\partial^2 x}{\partial t \, \partial s},$$

positive to the first term, and negative to the second term. The two terms then become differential coefficients with respect to s and t, and we get

$$\mathfrak{S} = \frac{\partial}{\partial s} \Sigma \mathrm{H}_x \frac{\partial x}{\partial t} - \frac{\partial}{\partial t} \Sigma \mathrm{H}_x \frac{\partial x}{\partial s}. \quad . \quad . \quad . \quad (37)$$

Finally, if in this we put for H_x and the two other quanti-

ties contained in the sums (H_y and H_z) their values determined by equations (16), we get

$$\mathfrak{S}=k\frac{\partial}{\partial s}\int\frac{i'}{r}\Sigma\frac{\partial x}{\partial t}\frac{\partial x'}{\partial s'}ds'-k\frac{\partial}{\partial t}\int\frac{i'}{r}\Sigma\frac{\partial x}{\partial s}\frac{\partial x'}{\partial s'}ds'. \quad . \quad (38)$$

This is the most convenient form of the expression of $\mathfrak{S}$ resulting from my fundamental law; and the product $\mathfrak{S}ds$ is the electromotive force induced in a conductor-element ds by a closed current or system of currents.

To obtain the corresponding expressions for Riemann and Weber's fundamental laws, we need only in the formulæ (28) and (29), representing the potential function, to take separately into consideration the added terms G_1 and G_2, which do not contain the velocity-components $\frac{dx}{dt}, \frac{dy}{dt}, \frac{dz}{dt}$, and therefore are to be differentiated only with respect to x, y, z. As we can now again form for G_1 the equation

$$\frac{\partial G_1}{\partial x}\frac{\partial x}{\partial s}+\frac{\partial G_1}{\partial y}\frac{\partial y}{\partial s}+\frac{\partial G_1}{\partial z}\frac{\partial z}{\partial s}=\frac{\partial G_1}{\partial s},$$

and for G_2 the corresponding equation, we obtain, denoting the electromotive force according to Riemann and Weber's fundamental laws by $\mathfrak{S}_1$ and $\mathfrak{S}_2$:—

$$\mathfrak{S}_1=\mathfrak{S}+\frac{\partial G_1}{\partial s}, \quad . \quad . \quad . \quad . \quad . \quad (39)$$

$$\mathfrak{S}_2=\mathfrak{S}+\frac{\partial G_2}{\partial s}. \quad . \quad . \quad . \quad . \quad . \quad (40)$$

These expressions represent very clearly the difference between the electromotive forces resulting from the three fundamental laws.

From the developments carried out in the last two sections it will, I think, be sufficiently apparent how much the introduction of the electrodynamic potential function of closed currents contributes to giving to the entire department of electrodynamics with which we are concerned a uniform character—the knowledge of that one quantity being sufficient, without any accessory assumption, for the derivation of every thing further by simple analytical operations.

XXXVI. *The Cause of the Production of Electricity by the Contact of Heterogeneous Metals.* *By* Prof. FRANZ EXNER *.

THOUGH it has so often been experimentally proved that a difference of electric potential is produced by the contact of heterogeneous metals, there has not as yet been given any explanation of this phenomenon that will bear scrutiny. The contact theory as well as the so-called chemical theory both lay claim to this phenomenon, and the adherents of the two theories are at the present day perhaps about equal. An investigation concerning the nature of galvanic polarization has led me to a quite distinct view of the origin of the so-called contact electricity, a view which will be verified by the following experiments. In the above-mentioned investigation I have shown that the original cause of the polarization-current is not to be looked for, as has heretofore been generally assumed, at the contact of the electrodes with the ions separated on them, but in the recombination of these latter; and the electromotive force of the current so produced is measured by the heat-value of this combination, just as the electromotive force of any galvanic cell is measured by the heat-value of the chemical process going on in it. With a so-called contact action the existence of the polarization current, and obviously of every other current, has nothing whatever to do.

When, therefore, the contact theory proved to be here perfectly inapplicable, and indeed superfluous, the idea at once suggested itself, to undertake from this standpoint a criticism of the first beginnings of this theory, *i. e.* the fundamental experiments of Volta; that is to say, the idea suggested itself to seek for the cause of the production of electricity at the contact of two metals, not in this contact, but in previous chemical actions of the surrounding media on the surfaces of the metals. Therefore, at the conclusion of the above mentioned paper on polarization, in referring to the analogy between tension series and oxidation series, I have expressed the opinion that the so-called contact electricity is produced by the oxidation of the metals in contact by the oxygen of the air, after the same laws according to which, in galvanic cells, electricity is evolved by the oxidation of the zinc. If this supposition prove true (and it has proved true), the electromotive force of two metals in contact in air must be measured and expressed by their heats of combustion.

The first attempt at a confirmation of this purely chemical theory consists, then, in a comparison between the electro-

* Translation of a paper in Wiedemann's *Ann. der Physik und Chemie*, 1880. Communicated by J. Brown, Esq.

motive force produced, and the heats of combustion of the metals.

This latter has been already published in the old experiments of Favre, Favre and Silbermann, and, in later times, in the comprehensive thermo-chemical researches of J. Thomsen. The data of this last author may well claim credit for the greatest exactitude and correctness; therefore in all the following calculations his figures only are taken.

Far less agreement exists between the works of various authors with regard to the electromotive force at contact of two metals; here, no doubt, the great difficulty of the experiments causes considerable differences. Nevertheless it follows, from the evidence of all known experiments, that the tension series is identical with the series of oxidability.

Even the first experiments of Volta * show this. Volta finds the tension series, with dry contact of the metals, Zn, Pb, Sn, Fe, Cu, Ag, Au, C, thus always descending from the easily oxidizable metals to the unoxidizable.

These, although only qualitative experiments, must yet draw attention to the part played by oxidation. For quantitative results without reference to any known unit of electromotive force, we have to thank Hankel †. He finds when the tension between Zn and Cu is 100:—

Al.	Zn.	Pb.	Hg.	Fe.	Cu.	Au.	Ag.	C.	Pt.
220	200	156	119	116	100	90	82	78	77

These numbers (at least with the exception of mercury) correspond with those mentioned above. Gerland ‡ obtained similar results. He finds, for instance, that, representing the difference of potential between Zn and Cu as $Zn|Cu$, $Zn|Cu=100$, $Zn|Ag=109$, $Zn|Au=115$.

When, however, we wish to have a distinct view of the cause of these differences of potential, it is certainly of the greatest importance to know, not only their relative, but also their absolute value—that is, the ratio they bear to a known electromotive force, *e.g.* that of a Daniell cell. This important determination was, so far as I know, first made by R. Kohlrausch; and since then unfortunately only isolated and unreliable observations have appeared.

Kohlrausch§ originated the following method:—He formed a condenser of the metals to be examined, connected it with the poles of a Daniell cell, first in one direction, then in the

* *Ann. de Chem. et Phys.* xl. p. 225.

† Pogg. *Ann.* cxxvi. p. 440, 1865; cxxxi. p. 607.

‡ Pogg. *Ann.* cxxxiii. p. 513, 1868.

§ Pogg. *Ann.* lxxxii. p. 407, 1851.

opposite, and measured, with a Delmann's electrometer, the electric charge in both cases. If the difference of potential of the two condenser-plates be P, and that of the poles of the cell D, then the condenser-charge for one connexion will be $P+D$, for the other $P-D$, from which the required ratio $\frac{P}{D}$ can be found. A direct connexion of the plates by a wire gives a check experiment.

In this way Kohlrausch finds the difference $Zn|Pt = \cdot 6$ Daniell, and the ratio $\frac{Zn|Pt}{Zn|Cu}$ varying between $\frac{106}{100}$ and $\frac{111\cdot 2}{100}$. We shall have an opportunity further on of returning to the probable cause of this variability.

Quite recently a number of other experiments bearing on this subject have been made, as, for example, by Clifton *, who likewise employed the condenser method. He finds :—

$$\begin{array}{lll} & Zn|Fe = \cdot 694 & \text{Daniell,} \\ & Fe|Cu = \cdot 095 & \quad " \\ \text{and from these} & Zn|Cu = \cdot 789 & \quad " \end{array}$$

by application of Volta's law of tensions, the truth of which will not be questioned.

The results of Kohlrausch appear to me, however, to possess greater accuracy. He finds, according to the above figures, Zn | Cu about ·5 Daniell. Ayrton and Perry † have obtained results generally analogous to those of Clifton.

Furthermore must be mentioned an old experiment by Sir W. Thomson ‡, giving an account of the absolute value of Zn | Cu. In his well-known ring electrometer the two half-rings were made of Zn and Cu. Over the slit swung a needle charged as might be desired. When the two half-rings were metallically connected the needle deflected to one side or the other; but if they were joined each to one pole of a Daniell cell in the proper manner, the force of the latter overpowered the contact force of the two metals, and it was possible, by a proper division of the electromotive force of the Daniell, to arrange so that the needle remained undeflected. Zn | Cu was consequently less than one Daniell, which agrees with all other experiments.

Thomson observed further, that when the copper half-ring was oxidized in the lamp-flame, its electromotive force with zinc rose considerably, and appeared to be even greater than that of 1 Daniell.

The foregoing data form, as it were, the basis of the two

* Proc. Roy. Soc. xxvi.
† Proc. Roy. Soc. xxvii.
‡ Proc of Manchester Soc. ii.

ruling theories of Volta's experiments. Still they give but a partial test for the same. It is known that Volta himself assumed the existence of a heterogeneous action of the touching metals on the electricity contained in them, in consequence of which a redivision of it resulted. This view, so far as I know, was first by Helmholtz brought into a determinate form, in which it is now generally accepted.

Helmholtz * says :—"All phenomena in conductors of the first class may be referred to the assumption that the different chemical substances have different affinities for the two electricities . . . . . . The contact force would therefore consist of the difference of the attraction forces which the molecules lying near the surface of contact exercise on the electricities at that place."

Against this position a protest, as is known, has been raised by Clausius †, at least so far as concerns the statement that "all phenomena in conductors of the first class" may be deduced from the contact force, referring principally to the phenomena of thermo-electricity.

After all, however, this hypothesis is at present commonly accepted; and it cannot be denied that through this concise theory any investigation in this field has rendered good service. It is another question whether, in the present state of things, we are compelled to hold with it, or if there may not be found a less forced explanation of voltaic phenomena, an explanation which shall accord with a series of other fully investigated phenomena.

And such is, as appears to me, the view advanced by the opponents of the contact theory, the founders of the chemical theory, viz. :— that two metals in contact are in a position to evolve electricity only when they are undergoing at the same time chemical change. This view endeavours, accordingly, to refer the production of electricity in Volta's experiments to the same causes as those at work in galvanic cells.

In fact, the correspondence between Volta's tension-series and the series of oxidation of the metals is so striking, that one is led at once to the supposition that the electric tension produced has its origin in the oxidation of the metals. This view was first perfectly developed by De la Rive; and he supported it by numerous experiments. How it was nevertheless subjected to numerous attacks, and how it gradually fell into oblivion, is well known.

De la Rive ‡ assumes that a metal in air is attacked, not

* *Ueber die Erhaltung der Kraft.*

† *Abhandlung.* xii.; and Pogg. *Ann.* xc. p. 513, 1853.

‡ *Traité de l'Éelectricité* ii.; and Pogg. *Ann.* xv. p. 98, 1828; xxxviii. 1836; xl. p. 515, 1837.

only by the condensed water-vapour, as is now generally supposed, but also in dry air directly by the oxygen, and, moreover, that electricity is produced by any kind of chemical action, in proportion to the intensity of the chemical affinity. I take it for granted that the nature and method of his deduction of the voltaic phenomenon from this hypothesis is well known.

If there had existed at that time exact quantitative measurements of the electromotive forces at contact, and also a measure for the intensity of chemical affinity such as we now have in the heats of combustion, the truth of this view, which is supported also by other writers *, would have been at once apparent.

Since such direct proof was not forthcoming, the opponents held fast the old Voltaic hypothesis with an unvarying bitterness, more especially Pfaff †, who sought, by experiment, to reduce the chemical theory to an absurdity. He arranged, *inter alia*, the fundamental experiment *in vacuo* instead of in air, also in indifferent gases such as hydrogen, but found always the same production of electricity, where none should have been expected. De la Rive has, however, proved the cause of this to be in the difficulty of removing a gas-film from the surface of a metal even *in vacuo*, and shown ‡, at the same time, that the experiment, when quite fairly carried out, was in favour of the chemical theory.

Nevertheless the chemical theory has in course of time been more and more supplanted by the contact theory, since an exact proof could not be found for either one or other ; and so the above-mentioned proposition of Helmholtz (originally expressed as a well considered hypothesis, and as such not to be undervalued) came gradually to be regarded as the expression of a fact.

Such was the position of matters when I was driven by my experiments on galvanic polarization to the conclusion that the so-called contact electricity had likewise a chemical origin. One can undertake the proof of this view in different ways:—first, by showing that two heterogeneous metals give no evolution of electricity when in a chemically indifferent environment. This proof has already been given by De la Rive in very careful experiments; and I consider it superfluous to repeat an experiment for which De la Rive is responsible. Secondly, it can be shown that the differences of potential assumed by any two metals in air have a direct connexion with the heats of combustion of the metals ; and,

* Compare for example E. Becquerel, *Compt. Rend.* xxii.

† *Ann. de Chim. et Phys.* xvi.

‡ *Ann. de Chim. et Phys.* xxxix.

thirdly, it can be shown that two pieces of one and the same metal evolve electricity immediately they are placed each in a differently acting atmosphere.

The second and third methods are followed in the following pages; and I shall now describe the experiments which show that the difference of potential between two metals in air is really measured by their heats of combustion.

In the first place it will be well to obtain a distinct conception with reference to the mode of dependence of the tension on the heat of combustion. Though the whole process of the production of electricity is, according to the present state of things, by no means clear, the theory contained in the following pages will be found to correspond in all important parts with the facts.

It is well known that in galvanic cells any chemical process gives rise to a difference of potential proportionate to its heat value; and in the case of the oxidation of a metal in air one must conclude that the difference of potential between the metal and the oxide formed is proportionate to the heat of combustion of the former. Thus any metal which, being insulated, is oxidizing in air must contain a certain amount of positive and of negative electricity separated from one another. That these must be inactive towards surrounding objects is obvious. Furthermore, the difference of potential of such separated electricities cannot exceed a certain limit; for the observed tension is always the same whether the oxidation continue or not. It appears therefore that the electricities evolved by further continued oxidation recombine with liberation of the corresponding heat equivalent.

If, for example, a piece of zinc has, by oxidation in the air, received the potential $+E$, but the oxide film or perhaps the enveloping air-film the potential $-E$, so that the difference of potential is $2E$, then this amount $2E$ is measured by the heat of combustion of the zinc. If now we connect the zinc with any metal unaffected by air, *e. g.* platinum, a part of the electricity of the zinc will pass to the platinum till both metals have the same potential, which we may call $+P$. The free tension at the zinc becomes now $-E+P$, that at the platinum $+P$; consequently the difference of potential between zinc and platinum is $-E$, and is thus measured by half the heat of combustion of the zinc.

If, accordingly, the heat-value of Daniell be A * and the combination of the zinc be B, the difference of potential between zinc and platinum in air must be equal to $\frac{B}{2A}$. If the

* The heat-values must all of course be referred to the chemically equivalent quantities of the substances.

metal in connexion with the zinc is also oxidizable in air, still the theory evidently remains the same; the difference of potential of two metals is always measured by half the difference of their heats of combustion.

Thus, in order to prove the truth of the chemical theory, one has only to compare the observed differences of potential with the heats of combustion so exactly determined by J. Thomsen.

Such a comparison shows at once a general correspondence between experiment and calculation, as also concerning the absolute value of the differences of potential. The figures, however, that have been given by different authors for the tension-differences (on account of reasons which we shall presently have opportunity to examine) agree only in a moderate degree. I have therefore endeavoured to obtain with all possible exactitude the difference of potential for at least a few metals. For this purpose I adopted exclusively the method of Kohlrausch. Thomson's method with the ring electrometer, though in principle the simplest, could not be used, because in the later experiments it was requisite to have the metals in different gases.

A condenser was accordingly formed from the metal to be examined and a thick plate of platinum, the latter insulated by paraffin from its metallic support. The metals examined were Zn, Fe, Cu, Ag, all in well-ground plates of 55 millims. diameter, as also the platinum plate. The insulating film of the condenser was of paraffin; and metal handles were fastened to the plates with the same material. The two poles of an insulated standard Daniell were now connected with the condenser-plates alternately in one sense and the opposite. The charges of electricity produced are proportionate to the sum and difference of the electromotive force of the plates and the Daniell, the ratio of which can therefore be ascertained from these two observations. As a check experiment the condenser was directly connected, and the resulting charge determined. The measurement of these quantities was made by a Branly's quadrant electrometer, the quadrants of which were charged by a Zamboni's pile. Its deflections were, according to a previous graduation within the prescribed limits, proportionate to the charges.

The first experiment related to the tension between pure zinc and platinum. I must remark that for the attainment of constant results it is absolutely necessary to clean and dry the plates well before every experiment, and also to work in a warm room. Also before every experiment the paraffin layer must be carefully tested, and, if it be electric, renewed by remelting. This, however, cannot of course be done between

two mutually-related experiments, since for such the distance between the condenser-plates must remain the same.

In what follows, N denotes the zero-point for each consecutive position of the telescope, a the observed deflection, reckoned positive when towards decreasing numbers, negative when towards increasing. The first vertical column contains the numbers of the experiments. Each horizontal series forms a complete experiment. A gives the reading with the condenser and Daniel acting in opposition to one another, B with them acting together; C the value of x and D calculated therefrom, where x represents the difference of potential of the metals under examination, and D the electromotive force of a Daniel, both expressed in scale-divisions. D gives the reading of the check experiment, and E the value found therefrom for x. F gives the mean of the values for x from C and E; and, lastly, G the value of x referred to a Daniell as unit.

I. Experiments with Zinc and Platinum.

	A.	B.	C.	D.	E.	F.	G.
1.	N=749 a=757	N=749 a=605	x=68 D=76	N=749 a=681	x=68	x=68	x=·90
2.	N=852 a=858·5	N=850 a=754	x=44·7 D=51·3	N=850 a=806·5	x=43·5	x=44·1	x=·88
3.	N=804 a=811	N=806 a=696	x=51·5 D=58·5	N=808 a=756	x=52	x=51·7	x=·88
4.	N=857 a=864	N=857 a=766	x=42 D=49	N=857 a=814	x=43	x=42·5	x=·86
5.	N=668 a=675	N=668 a=556	x=52·5 D=59·5	N=668 a=615	x=53	x=52·7	x=·88
6.	N=674 a=680	N=674 a=552	x=58 D=64	N=676 a=618	x=58	x=58	x=·91
7.	N=714·5 a=724·5	N=715 a=655	x=75 D=85	N=714 a=639·5	x=74·5	x=74·7	x=·88
8.	N=858 a=865	N=858 a=762	x=44·5 D=51·5	N=855 a=810	x=45	x=44·7	x=·87
9.	N=848 a=855	N=848 a=747	x=47 D=54	N=847·5 a=801	x=46·5	x=46·7	x=·86
10.	N=720 a=730	N=720 a=560	x=75 D=85	N=718 a=644	x=74	x=74·5	x=·88
11.	—	N=808 a=694	D=60	N=806 a=752	x=54	—	x=·90
12.	—	N=808 a=695	D=60	N=808 a=755	x=53	—	x=·88
13.	N=855 a=871·5	N=855 a=757	x=45·7 D=52·3	N=853·5 a=808	x=45·5	x=45·6	x=·87
						Mean...	x=·881

In each of the experiments 11 and 12 a check observation is omitted. The experiments 10 to 13 refer in reality to zinc and gold, as in them the platinum plate was replaced by a thickly-gilded brass one. It is evident that gold and platinum behave quite similarly. I must, however, here remark that this equality did not continue long. Even after two or three days the gold plate gave considerably smaller values than the platinum ; and it is not long till such a gilt brass plate behaves almost the same as one without gilding. This can only be due to the fact that the air gradually permeates the gold, and oxidizes the brass underneath.

A similar observation has been made by De La Rive with varnished plates, so long as the varnish layer has not a very considerable thickness. This behaviour of a thin metallic covering is of interest, because the most apparently trustworthy of the old experiments were made with such gilt and platinized plates, for example those of Kohlrausch.

This author arranged special experiments * to try if a platinized brass plate behaved the same as a thick platinum one, and found that it did so. Still this experiment was in all probability tried soon after the platinizing; and I think I do not err in ascribing the smallness of the value for Zn|Pt which Kohlrausch obtained, viz. ·6 Daniell, instead of ·88, to a gradual permeation of air through the platinum covering.

This is confirmed by the fact that Kohlrausch found the ratio $\frac{\mathrm{Zn|Cu}}{\mathrm{Zn|Pt}}$ very variable, and also (in comparison with the observations given below) too large, viz. $\frac{100}{111 \cdot 2}$ to $\frac{100}{106}$. If the observed value Zn|Pt was too small, this ratio would evidently be too great.

I could not find that any other author besides Kohlrausch had given any data on the absolute value of Zn|Pt.

In order now to obtain an idea of the correctness of this value, or perhaps of the chemical theory itself, we have only to divide half the value of the heat of oxidation of zinc by the heat-value of the Daniell cell. According to J. Thomsen the heat of oxidation of zinc is 42,700 calories per equivalent ; and the heat-value of the Daniell cell is similarly calculated from Thomsen's figures equal 24,300 calories. From these the value of Zn|Pt is found to be ·879 Daniell. This number corresponds with observations better than could have been expected with such difficult experiments.

* Pogg. *Ann.* lxxxii. p. 407, 1851.

II. Experiments with Copper and Platinum. Mode of observation and arrangement of table same as before.

	A.	B.	C.	D.	E.	F.	G.
1.	N=679 a=729	N=679 a=565	x=32 D=82	N=680 a=649	x=31	x=31·5	x=·38
2.	N=681 a=735	N=681 a=565	x=31 D=85	N=680 a=649	x=31	x=31	x=·35
3.	N=701 a=750	N=701 a=592	x=30 D=79	N=703 a=672	x=31	x=30·5	x=·39
4.	N=709 a=770	N=709 a=575	x=36·5 D=97·5	N=709 a=672·5	x=36·5	x=36·5	x=·37
5.	N=709 a=768	N=709 a=577	x=36·5 D=95·5	N=709·5 a=674	x=35·5	x=36	x=·38
6.	N=707 a=769	N=707 a=577	x=34 D=96	N=707 a=673	x=34	x=34	x=·35
7.	N=708 a=770	N=708 a=578	x=34 D=96	N=708 a=674	x=34	x=34	x=·35
							Mean... x=·367

These experiments do not agree with one another so well as those with zinc. I have generally found that, of all metals, zinc gives by far the most constant results. The heat of oxidation of copper is, according to Thomsen, 18,600 calories; and from this is calculated as above the value of Cu | Pt=·383 Daniell, which agrees sufficiently well with experiment.

The fact that in different experiments the absolute values for x and D are not quite equal is because between almost every two experiments the paraffin layer was either completely melted or, at all events, gone over with a Bunsen flame.

III. Experiments with Iron and Platinum. Method of observation and arrangement of table same as in I. and II. With iron especial caution is required, and it is imperatively necessary that the plates be newly cleaned before every experiment.

	A.	B.	C.	D.	E.	F.	G.
1.	N=800 a=812	N=800 a=720	x=34 D=46	N=800 a=766	x=34	x=34	x=·75
2.	N=822 a=834	N=820 a=741	x=33·5 D=45·5	N=820 a=787	x=33	x=33·2	x=·73
3.	N=795 a=810	N=790 a=713	x=31 D=46	N=790 a=759	x=31	x=31	x=·68
4.	N=830 a=844	N=830 a=746	x=35 D=49	N=830 a=795	x=35	x=35	x=·71
5.	N=758 a=772	N=758 a=680	x=32 D=46	N=760 a=729	x=31	x=31·5	x=·68
6.	N=712 a=727	N=712 a=627	x=35 D=50	N=715 a=680	x=35	x=35	x=·70
7.	N=681 a=699	N=682 a=586	x=39 D=57	N=680 a=641	x=39	x=39	x=·68
8.	N=698 a=713	N=699 a=613	x=35·5 D=50·6	N=700 a=665	x=35	x=35·2	x=·70

Since the heat of combustion of iron is 34,100 calories, calculation gives for Fe | Pt the value ·701 Daniell, which agrees perfectly with the observed value.

Finally silver was submitted to experiment; but, on account of its small heat of oxidation, its difference of potential with platinum is so small that the smallest error of observation modifies considerably the result. I have therefore examined silver only in order to be able to make a direct comparison with the experiment given further on relating to silver in a chlorine atmosphere. For the value Ag | Pt in air I have only made one measurement, which, on account of the said comparison, may be given here. The plate used was a solid silver plate; for galvanoplastic silver films possess a permeability for air in a yet higher degree than gold films.

The notation being as before, we get:—

A.	B.	C.	D.	E.	F.	G.
N=800 a=855	N=800 a=735	x=5 D=60	N=800 a=795	x=5	x=5	x=·083

Since, according to Thomsen, the heat of combustion of silver is 3000 calories, the value Ag | Pt comes out ·062 Daniell, which, taking into account the smallness of the value, corresponds sufficiently well with the experiment.

I now tried the experiment of surrounding the two metals of the condenser with different gases; and for this purpose I chose both plates of the same metal, viz. silver, allowing air to act on one, but chlorine gas on the other. For this experiment the apparatus must be arranged as follows:—A short cylindrical glass tube was closed air-tight at one end with one of the silver plates, but so that the plate did not touch the glass anywhere, a condition easily attained by cementing with paraffin. The lower end of the vertically placed glass tube was closed air-tight by a stopper, through which passed two small glass tubes for the entrance and exit of the gas, and also a platinum wire thoroughly insulated by paraffin, the inner end of which pressed against the silver plate. This latter was merely for the purpose of making the metallic connexion of the condenser. The silver plate was now covered on its outer surface with paraffin, which formed the insulating layer of the condenser.

A second silver plate, of the same dimensions as the first, could be placed on this condenser exactly as in the earlier experiments.

If now the condenser-plates were connected, there was of course not the slightest charge apparent. As soon, however,

as the interior of the glass tube was filled with dry chlorine gas, evolved from potassium bichromate and hydrochloric acid, the condenser showed at once a considerable and quite constant tension. At the same time the inner side of the lower silver plate blackened. The difference of potential lasted, however, only so long as the combination of the chlorine with the silver went on. If the chlorine was driven out of the vessel by dry air, and time allowed for the silver to completely consume the remaining traces of chlorine clinging to it, no further difference of potential between the clean and the attacked plates was apparent.

Below I give the results of these experiments:—

IV. Silver in Air with Silver in Chlorine. The notation is again the same as before.

	A.	B.	C.	D.	E.	F.	G.
1.	N=800 a =789	N=800 a =840	x =14·5 D=25·5	N=800 a =815	x=15	x=14·7	x=·57
2.	N=803 a =792	N=802 a =842	x =14·5 D=25·5	N=802 a =816	x=14	x=14·2	x=·56
3.	N=690 a =676	N=690 a =734	x =15 D=29	N=690 a =704	x=14	x=14·5	x=·50

It will be seen from the figures under A, B, and D that the deflections are now all in the direction opposite to that of the former experiments, because now, instead of connecting, as at first, the more strongly attacked plate to the electrometer, the less strongly attacked one was connected.

Yet another experiment was arranged as follows:—The plate which closed the tube was taken out, cleaned perfectly, newly polished, and replaced. Again the two plates gave no trace of a difference of potential. The lower plate was now allowed to remain in contact with air, but the movable one was held for a short time in a stream of chlorine and immediately examined. The following values were found:—

A.	B.	C.	D.	E.	F.	G.
N=658 a =683	N=658 a =573	x =30 D=55	N=658 a =628	x=30	x=30	x=·54

The deflections are here again in the same direction as those of the Zn \| Pt condenser, since now again the more strongly attacked plate is connected to the electrometer. The mean of the foregoing experiments is ·542.

According to Thomsen, the heat of oxidation of silver is 3000 calories, and the heat of combination of chlorine and

silver is 92,400 calories. Therefore the calculation for the difference of potential between silver in air and silver in chlorine gives the value ·543 Daniell. This extraordinary agreement, considering the small number of experiments, can probably only be ascribed to chance. Finally, the movable silver plate was exposed to the action of chlorine till perfectly black, and again examined. There resulted the value ·54, as before; but this value decreased very rapidly when the plate continued exposed to the air; and experiments immediately following one another gave the values ·46, ·42, ·38, ·36; and, after standing twelve hours, no difference of potential was observable, although the one plate was bright and the other perfectly blackened by chlorine.

I believe the results of the foregoing experiments bear eloquent testimony in favour of the chemical theory. It is not only that the qualitative relations correspond, without exception, to this hypothesis, but the quantitative determinations agree so well with the calculated values that the truth of the theory under consideration hardly admits of any further doubt. The only substance with which I have not succeeded in obtaining any positive result was lead. Not that it failed, when in contact with platinum, to give values which agreed with calculation, but it was impossible to obtain two observations that would agree together. The reason of this was apparently the great rapidity with which a bright lead surface is attacked by the air, and, secondly, the impossibility of giving it a good polish; this latter, however, is imperatively necessary when working with a solid insulating layer in the condenser, in order that the distance between the plates may be the same at each measurement.

In reference to the value of the last experiments with silver in chlorine, I may make the following remarks:—If one could prove, with regard to all substances, that their differences of potential are proportionate to the differences of their respective heats of combination, this would still be no direct proof against the contact theory; such, however, is given by the experiments with silver.

The contact theory evidently loses its basis so soon as it is shown that two heterogeneous metals in contact assume no difference of potential. This De la Rive has already shown by his experiment with different metals *in vacuo*, an experiment which has unfortunately fallen into oblivion. The same result can, however, be attained more easily and in a more obvious manner.

If two bright copper plates be in contact in air, they show no difference of potential,—according to the contact theory, because they are the same substance; according to the chemical theory, because the action of the air is equal on both. If now this action be removed from one plate, *e. g.* by oxidation in a flame, it is well known that a great difference of potential is obtained—which, according to the chemical theory, is because only one of the plates now continues to oxidize in the air and the other not; according to the contact theory, because now two substances are brought in contact, viz. copper and copper oxide. Which of the two explanations is correct is decided by the above experiment with silver in chlorine. In the case where one of the silver plates is in air and the other in chlorine, there is a certain difference of potential so long as the action of the chlorine continues. As soon, however, as both plates are surrounded by air, the difference of potential disappears altogether, since now the action on both plates is again the same. This experiment with silver and chlorine succeeds well, because the silver takes but a short time to consume the film of chlorine clinging to it.

According to the chemical theory, the explanation of this experiment is perfectly clear; but according to the contact theory, not so. According to the latter, the difference of potential should continue, since one plate is silver and the other silver chloride, or at least is covered with it. In fact, I see no way of bringing this experiment in accord with the contact theory.

While I was engaged in carrying out the foregoing work, there appeared two papers by Brown * on the same subject which I cannot here pass unnoticed. Unfortunately Brown was not in a position to make quantitative measurements with his apparatus; still it is always remarkable when investigators are led from quite different sides and quite independently to the same result. Though priority at least in carrying out the idea qualitatively certainly belongs to Brown, yet I believe I am the first to have placed the matter by quantitative proof in the true light.

Reviewing now the results of all researches on contact electricity up to the present time, we shall find, as it appears to me, no argument capable of being held at all against the

* Phil. Mag. [5] vi. August 1878, and vii. February 1879. A short abstract describing the results obtained by Brown is omitted here, as the two papers can easily be referred to.

chemical theory, but very weighty reasons in its favour and against the voltaic theory.

I believe we are perfectly entitled to say that a development of electricity by the contact of heterogeneous metals and an electrical parting force (*Scheidungskraft*) at the surface of contact of two heterogeneous metals *does not exist.* The following must take the place of Volta's law of the evolution of electricity:—"The difference of electric potential between two metals in contact is measured by the algebraic sum of the heat-value of the chemical action going on at each." This law holds equally good for every galvanic cell as well as for galvanic polarization and Volta's fundamental experiment.

That Volta's law of tensions is not prejudiced thereby, but, on the contrary, is an immediate consequence of the chemical theory, requires no explanation.

It is evident that such a transformation of the theory of the production of electricity cannot be without effect on a large number of phenomena, to only one of which, however, I shall here refer, viz. thermo-electricity. There has been proposed by Le Roux *, and defended by many others, a theory that the origin of the thermo-electric effect is to be found in an alteration of Volta's contact force caused by the variation in temperature. This view has latterly been supported, among others, by Edlund †, who endeavoured to refer the voltaic tension between two metals to their thermo-electric relations; also by Avenarius ‡ and by Gaugain §. This view, however, according to the foregoing experiments is highly improbable, if not untenable. We can on no account accept the totally unfounded proposition that the heats of combustion of the metals are materially altered by even the smallest variations of temperature, and, secondly, that the difference of such alteration varies considerably in different metals. Furthermore, the thermo-electric force of the two metals must vary considerably according to the medium in which they are immersed, a phenomenon which, if it existed at all, could hardly have escaped observation till now. It seems to me, therefore, far more probable that the thermo-electric force has nothing whatever in common with the so-called voltaic force. Moreover it has already been shown by Kohlrausch ‖ that the assumption of such a connexion is entirely unneces-

* *Compt. Rend.* lxiii.

† Pogg. *Ann.* cxliii. p. 404, 1871.

‡ Pogg. *Ann.* cxix. p. 406, 1863; cxxii. p. 193, 1864.

§ *Ann. de Chim. et de Phys.* [6] vi. p. 193, 1864.

‖ Pogg. *Ann.* clvi. p. 601, 1875.

sary, and that we may form quite a distinct idea of the nature of thermo-electricity when viewed from a different standpoint.

When we consider that the theory of the galvanic cell as at present existing rests altogether on the assumption of voltaic contact force, we may well expect the most radical changes in this direction; and, so far as I can at present see, the theory of the cell may be represented in a considerably simpler manner and on more natural principles than heretofore.

The results of a more searching investigation of these matters shall be reserved for future publication.

XXXVII. *Intelligence and Miscellaneous Articles.*

THE COLOURS OF THIN BLOWPIPE DEPOSITS. BY C. H. KOYL, B.A., STUDENT OF PHYSICS IN JOHNS HOPKINS UNIVERSITY.

SOME examples of the action of very fine particles of matter upon light having lately come to my notice, it may be interesting to make them public, as they have heretofore, I believe, been unexplained.

Those who are familiar with the methods of blowpipe analysis have observed faint borders occasionally surrounding some of the coloured charcoal coatings, the colours of these borders seemingly bearing no relation to the characteristic colours of the adjoining oxides. For instance, the white coating of antimony is generally accompanied with a blue border, the brownish oxide of cadmium occasionally with a green, while the lead and bismuth yellows not unfrequently have a whitish ring inclosing them. As these occur only and always where the coating is very thin, they have a significance different from that of the ordinary colours, and as they may be produced at pleasure from the purest specimens, they cannot be due to mixtures of the metals. A possible analogy with the antimony blue was suggested by a consideration of the colours of the sky; and to prove the connection, it was simply necessary to show the similarity of attendant phenomena. As is well known, it is believed that the blue of the sky is due to the presence in the atmosphere of suspended particles so fine that they are unable to reflect the longer rays of the spectrum, which accordingly are transmitted, and the union of the remainder gives to the sky its blueness. At evening the sky is red because we get the rays of the sun directly transmitted or reflected from the clouds. Thirdly, the light of the sky, reflected at an angle of 90° with the sun, is plane-polarized.

When an antimony coating had been produced which gave, beyond the white oxide, a blue well defined and full, the whole was illuminated in a dark room by a sodium flame; and that the blueness was no psychical or physiological effect as distinguished from ordinary vision, was proved by the fact that here it almost

completely vanished, while the white presented the usual ghastly appearance. A blue book-cover, treated in the same manner, gave more reflection than did the blue coating.

Experiments with the polariscope were at first inconclusive, from the fact that though the light from the blue coating was largely polarized, so, to some extent, was also that irregularly reflected from the charcoal, and it was found necessary to cover the block with a thin layer of carbon from a gas-flame. The repetition of the test then showed that the proportion of light polarized by the layer of carbon, at the given angle, was almost nothing; that by the thick white coating, small, while on the blue the phenomenon was almost complete. What light here was not polarized was evidently reflected from the larger particles mixed with the fine; for the analyzer, while it did not totally extinguish the light, yet excluded nearly all appearance of blueness.

In order to determine the character of the transmitted light, a microscope covering-glass was inlaid in the charcoal and the oxidation so executed that the glass was in the centre of a small area, all of which was blue. On removing the glass, the light which passed through proved to be of the expected yellow, though less brilliant than anticipated. The colour might be seen either by transmitting the direct light of the sun, or by placing the glass at such an angle that total reflection was produced, and thus in the passage of the rays through the layer to the glass and out through the layer to the eye the blue was principally lost and only the mixture of longer rays appeared. Viewed through a microscope, the result was the same. I have since, however, improved upon this plan by the more convenient method of covering with carbon a piece of ordinary window-glass, three inches by two, and then projecting the oxide upon the *opposite* surface of the plate. There is thus no difficulty in distinguishing a very slight amount of colour in the coating; and for transmitted light any portion of the carbon may be easily removed.

This case, a type of all charcoal coatings which shade off to blue in thin layers, appears thus parallel to that of the sky-colour; and the theory which is accepted for the one will also satisfactorily explain the other.

To account for the cadmium-green we have only to note that, if the substance upon which we are experimenting have the power of absorbing the shorter rays of the spectrum, the reflected light would from a heavy coating be yellowish or reddish, the particular shade depending upon the amount of absorption of violet and blue, and the formation of a layer as thin and of particles as fine as before should result in giving us the colour of the shortest rays which the substance is capable of reflecting, viz. in this case, green. The coating of cadmium has exactly this appearance, and shows the effect of the gradual transmission of red by shading from the original colour (dark red) through yellow into a fine green. As before, the light reflected from the thin layers is highly polarized, and the rays which pass through form a deep, dark red.

In exceptional cases it is possible to produce such a thin coating that the extreme edge is fringed with a faint blue.

The other case, lead, is now easily explained. This metal gives a coating of which the colour is a beautiful chrome yellow; and regarding this merely as a repetition of the preceding phenomenon, and the yellow as compounded of rays from the whole range of the spectrum but not in the proper proportion to form white, the line of thought suggested evidently is that, if the layer be decreased in thickness regularly from the centre to the circumference of the charcoal, there ought to be, at some distance from the centre, a zone within which sufficient red should be transmitted to equalize the amount of blue lost by absorption, and the reflected rays should form a yellowish white. Beyond this, as the thickness of layer still decreased, the colour should be blue for the same reason as in the case of antimony. The white zone is easily produced; and the blue border which always surrounds it polarizes the light as before and transmits orange-coloured rays.

The theory, once given, serves to explain nearly all the anomalous colourings of the charcoal coatings, the bluish borders which occasionally skirt almost any of the metallic oxides, the "peacock-tails" of cadmium, etc., and thus does away with the necessity of supposing the presence of impurities (though, by the way, no impurity would solve the problem in the case of the cadmium green.)

From a physical standpoint, the experiments seem interesting as an extension of our knowledge of the action of these small particles upon light. Had not the subject presented itself in this way, we would scarcely have guessed that such a change in reflecting-power could have been produced by so small a change in size and thickness.—Silliman's *American Journal*, September 1880.

Baltimore, Md., July 9, 1880.

ON AN AREOMETER FOR DETERMINING THE DENSITY OF SOLID BODIES. BY M. BUGUET*.

The author makes the rod of a Nicholson's areometer thicker and longer than it usually is, denotes by o and n the depth to which it sinks when unloaded and when loaded with n grams, and graduates the interval into parts corresponding to cubic centimetres and their subdivisions. If, when the body to be investigated is on the upper pan the areometer sinks to the division-mark P, and when on the lower to P′, the specific gravity is $\frac{P}{P-P'}$.—Wiedemann's *Beiblätter*, 1880, No. 7, p. 497.

DETERMINATION OF THE SPECIFIC GRAVITY OF SMALL FRAGMENTS OF MINERALS. BY J. THOULET†.

A solid body is pressed into a small ball of wax, so that the mean

* *Journ. de Phys.* ix. pp. 93, 94 (1880).

† *Z.-S. f. Kryst.* iv. p. 421 (1880); *Bull. Soc. Min.* ii. p. 189 (1879).

specific gravity amounts to 1–2. The fragments of the mineral are stuck upon the ball and the whole put into a solution of iodide of mercury in potassium iodide. The latter is then diluted till the wax ball just floats in it. If P, V, D and p, v, d are respectively the weight, volume, and density of the float and of the substance to be investigated, and Δ the specific gravity of the liquid, then

$$d=\frac{p\Delta}{\mathrm{P}+p\Delta\mathrm{V}}.$$

Wiedemann's *Beiblätter*, 1880, No. 7, p. 497.

ON THE LAW OF MAGNETOELECTRIC MACHINES. BY J. JOUBERT.

I recently had the honour of communicating to the Academy* the experimental methods which I employ in order to study the laws of the alternating currents used for the production of the electric light. The application of those methods to Siemens's alternate-current machine has shown me that the mean intensity of the current given by that machine is very accurately represented by the formula $\mathrm{I}=\frac{\mathrm{C}}{(\mathrm{R}^2+m^2)^{\frac{1}{2}}}$, in which R is the total resistance of the circuit, m a constant depending only on the velocity and varying in the inverse ratio of the duration T of the period; and C is another constant, equal to the quotient by $\sqrt{2}$ of the maximum value of the electromotive force of the machine working with open circuit, measured directly.

The simplicity of the result, and the complete concordance of the experiments with the formulæ, made me think I had before me not merely an empiric formula, but the expression itself of the law of the phenomenon; and I was led to try whether the theory could not conduct me back to that formula.

Let us suppose that the motion of the machine is uniform. Let E be the value, at a given moment, of the electromotive force resulting from the primitive magnetic field—that is to say, of the field as it exists when the induced system is at rest; and let I be the quantity of electricity set in motion during the time dt, starting from that moment. The electromagnetic work is equal to $\mathrm{EI}dt$, and is found again in the thermal work of the current $\mathrm{I}^2\mathrm{R}dt$ and in the work of the inverse electromotive forces which spring from the reactions of the various parts of the machine. Experience shows that the reactions upon the inducing electromagnets are negligible; for the current of the excitatrix, measured by an extremely sensitive galvanometer, shows no variation when the induced circuit is closed or when it is opened; therefore the reactions are reduced to the induction of the current upon itself. If we represent by U the flow of force emanating from the induced system when it is traversed by the unit of current, and, consequently, by $\frac{\mathrm{U}}{2}$ what is called the

* *Comptes Rendus*, July 26, 1880.

coefficient of self-induction, the work of the extra current during the time dt has the value $\mathrm{UI}\frac{dI}{dt}dt$. We have, then, the equation

$$\mathrm{EI}dt = \mathrm{I}^2 dt + \mathrm{UI}\frac{dI}{dt}dt,$$

or, dividing by I and by dt,

$$\mathrm{E} = \mathrm{IR} + \mathrm{U}\frac{dI}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This equation is no other than that given by Helmholtz, from which he deduced the laws of the induced currents which are produced at the moment of the closing and of the opening of the circuit of the pile—with this difference, however, that in M. Helmholtz's formula the quantity E is a constant, while here it is a function of the time.

To determine this function I operated as follows:—I put the induced system into communication with a Thomson galvanometer with the oscillations not deadened; and, the arc corresponding to the half-period of the machine having been divided into ten equal parts, by means of a very simple arrangement I caused the induced system to run over abruptly in succession the ten consecutive intervals. The arc of impulsion of the galvanometer measures each time the total quantity of electricity set in motion, and consequently the electromotive force corresponding to the successive displacements. The electromotive force thus measured is certainly that resulting from the primitive field, since in each displacement the quantities of electricity due to the reactions have a sum identically *nil*. The curve thus obtained does not sensibly differ from a sinusoid; we can therefore assume that E is of the form $\mathrm{E}_0 \sin mt$, the time being counted from the moment when the axis of the induced coincides with that of the inducing coil. Under these conditions, and putting

$$\tan 2\pi\phi + \frac{2\pi}{\mathrm{T}}\frac{\mathrm{U}}{\mathrm{R}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the integral of equation (1) can be written

$$i = \frac{\mathrm{E}_0}{\left(\mathrm{R}^2 + \frac{4\pi^2\mathrm{U}^2}{\mathrm{T}}\right)^{\frac{1}{2}}} \sin 2\pi\left(\frac{t}{\mathrm{T}} - \phi\right) = \mathrm{A} \sin 2\pi\left(\frac{t}{\mathrm{T}} - \phi\right),$$

the constant being determined by the condition that $t = \phi\mathrm{T}$ when the intensity is *nil*. The intensity of the current at each instant is therefore represented by a sinusoid, of which A is the amplitude and ϕ the phase.

The total quantity of electricity which passes in the circuit during half a period has for its value

$$\phi = \int_{t=\mathrm{T}\phi}^{t=\phi\mathrm{T}+\frac{\mathrm{T}}{2}} i dt = \mathrm{A}\int_{t=\phi\mathrm{T}}^{t=\phi\mathrm{T}+\frac{\mathrm{T}}{2}} \sin(mt - 2\pi\phi)dt \frac{\mathrm{AT}}{\pi}; \quad . \quad . \quad (3)$$

and we find, for the mean intensity I,

$$I=\frac{2A}{\pi}=\frac{\frac{2E}{\pi}}{\left(R^2+\frac{4\pi^2U^2}{T^2}\right)^{\frac{1}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The electrometer, in the conditions in which I use it, does not give this mean intensity, but the square root of the mean of the squares of the intensities—that is to say, an intensity I′ satisfying the condition

$$I'^2\frac{T}{2}=A^2\int_{t=\phi T}^{t=\phi T+\frac{T}{2}}\sin^2(mt-2\pi\phi)dt=\frac{A^2T}{4}.$$

From this we deduce

$$I'=\frac{A}{\sqrt{2}},$$

and consequently

$$\frac{I'}{I}=\frac{\pi}{2\sqrt{2}}=1\cdot 11.$$

The formula to be compared with the experiments is therefore

$$I'=\frac{\frac{E_0}{\sqrt{2}}}{\left(R^2+\frac{4\pi^2U^2}{T^2}\right)^{\frac{1}{2}}},$$

—that is to say, the formula to which I had been empirically conducted.—*Comptes Rendus de l'Académie des Sciences*, Sept. 6, 1880, t. xci. pp. 468–470.

ON AN ACOUSTIC METHOD OF DETERMINING VAPOUR-DENSITIES. BY H. GOLDSCHMIDT*.

From Laplace's formula for the velocity of sound in gases there results for the ratio of the densities of two gases d and D which, successively set in vibration in the same tube, give tones of the vibration-periods n and N,

$$d : D=N^2 : n^2.$$

For D=1 (air),

$$d=N^2 : n^2=\text{air-tone}^2 : \text{gas-tone}^2.$$

The author raps the test-tube filled with the gas in question, and seeks the resulting tone upon a violin. This procedure is applicable also to substances which are liquid at ordinary temperature. The test-tube, containing a small quantity of the substance, is closed above with a caoutchouc stopper through which passes a capillary tube, and brought to evaporation in steam. When no more vapour issues from the capillary tube, the stopper is pulled out and the tone then heard is determined. The observed agree very well with the calculated values.—Wiedemann's *Beiblätter*, 1880, No. 7, p. 500.

* *Chem. Ber.* xiii. pp. 768–771 (1880).

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[FIFTH SERIES.]

NOVEMBER 1880.

XXXVIII. *On the new Action of Magnetism on a permanent Electric Current.* *By* E. H. Hall, *Assistant in Physics at the Johns Hopkins University*.*

In the early part of last winter there was published in the 'American Journal of Mathematics'† an account of some experiments which prove that an electric current, as distinguished from the conductor bearing the current, is acted upon by magnetic force in a manner altogether different from that in which ordinary induction is known to take place. The new phenomenon was, in short, the action of a permanent magnetic force on a permanent electric current. Up to the time when the above-mentioned article was written, this new action had been observed only in one conducting material—gold. In the present article will be given the results of observations with several other conductors; but first it seems worth while to give some account of various closely related experiments which, though resulting negatively, are not entirely devoid of interest.

In the previous article the fact was mentioned that a form of apparatus had been devised which, it was thought, might reveal the new action in the shape of an increase of resistance in the conductor. The plan, as modified in accordance with a

* Printed from a separate impression, communicated by the Author, of the paper in Silliman's American Journal for September 1880. In its original form this article was a thesis for the degree of Doctor of Philosophy. Some alterations have been made in preparing it for publication.

† Vol. ii. p. 287 (1879); republished in the Philosophical Magazine for March 1880.

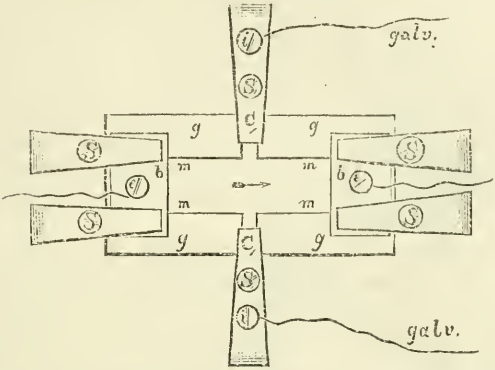
suggestion of Professor Rowland, was to employ as the conductor to be experimented upon a circular disk of gold leaf, in which the current entering at the centre would radiate to a thick ring at the edge, and so pass off by a wire attached to the ring. In such an apparatus under ordinary conditions the electromotive force, and so the flow of electricity, would be along the radii of the disk; but if a strong magnetic force were made to act perpendicularly to the face of the disk, a new electromotive force would be set up, which would be always perpendicular to the direction of the magnetic force and to the actual direction of flow of electricity at any instant in every part of the disk. The actual electromotive force under which the electricity would flow would therefore be compounded of two, one of which would in general have the direction of the radii of the disk, while the other would be nearly at right angles to this, though changing its direction constantly as the flow of electricity continually veered from its normal course under the resultant action of the two electromotive forces. The resulting path of the electricity from the centre to the circumference of the disk would be, not a straight line as under normal conditions, but a spiral. This path being longer than the straight line, we should expect an increase of electrical resistance in the disk of gold leaf. Before any very extended experiments had been made with this apparatus, however, it was pointed out by Professor Rowland that the increase of resistance which might be looked for in this case would be exceedingly small, probably too small to be detected. This experiment was therefore abandoned, for the time at least.

The next experiment to be described was a very simple variation upon the main one. And before going further it may be well to give a drawing of such a plate as has been used in making most of the observations to be hereafter recorded.

In fig. 1, which is about one half the actual size of an ordinary plate, $g\,g\,g\,g$ represents the plate of glass upon which the metal strip $m\,m\,m\,m$ is mounted. Contact with this strip is made at the ends by the two thick blocks of brass $b\,b$, which are held firmly in place by the four brass clamps worked by means of the screws S, S, S, S. The main current of electricity enters and leaves the metal strip by means of the binding-screws $e\,e$. Running out from the middle of this strip are two projections which make contact with the clamps $C_{\prime\prime}$, $C_{\prime\prime}$, worked by the screws $S_{\prime\prime}$, $S_{\prime}$. From the screws i, i, wires lead to the Thomson galvanometer. The projections from the metal strip just alluded to make the apparatus very easy to adjust; for by scraping off little particles from the proper part of the projections while the current is allowed to run through the metal

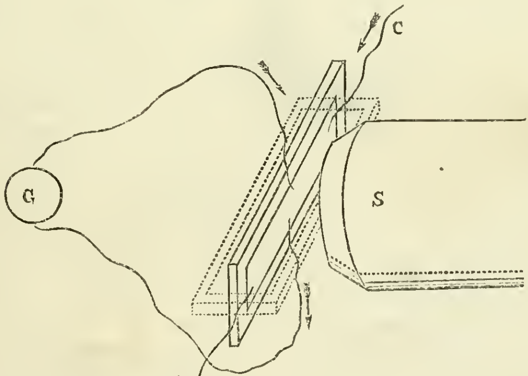
strip, the current through the Thomson galvanometer may be reduced to the extent desired.

Fig. 1.



In ordinary experiments such a plate as that just described is placed between the poles of the magnet in such a position that the direction of magnetic force would be represented by a perpendicular to the plane of the paper in the above drawing.

Fig. 2.



In the variation upon the main experiment a plate was employed similar to the above, but narrower, and with very short side clamps. This plate was first placed between the poles of the magnet in the usual position, as shown by the heavy lines in fig. 2.

With this arrangement a permanent deflection of about 30 centims., on the scale before the Thomson galvanometer, could

be obtained by reversal of the magnet current. Leaving now the distance between the poles very nearly the same as before, and using, both in the magnet and the gold strip, as nearly as possible the same strength of current which had just been employed in the previous trial, the plate was turned into the position indicated by the dotted lines in fig. 2. With this second arrangement no action of the kind previously seen was detected, or at least none that could with certainty be distinguished from the direct action of the magnet on the Thomson galvanometer. This latter effect produced a deflection of only a few millimetres, and could not have masked any considerable action of the kind looked for.

The first part of this experiment, then, shows our main fact, viz. that, in a conductor subjected to the given conditions, a permanent electromotive force is at once established, which has a direction perpendicular to the direction of magnetic force and perpendicular to the direction of the primary current in the conductor. The second part of the experiment shows that, under similar conditions, no electromotive force is set up in the direction of the magnetic force, or at least none of the same order of magnitude as that described above.

The third experiment to be described was made at the suggestion and desire of Professor Rowland. It was to test for an action of the magnet on the lines of static induction in glass. A thick piece of plate glass about 4 centims. square was taken, and a hole about 4 millims. in diameter was drilled through each of the four lateral faces. These four holes were all directed toward the centre of the glass; and each extended to within about 7 millims. of this point. If the holes had met, they would have formed two cylindrical channels at right angles to each other, and extending straight through the glass from lateral face to lateral face. In each hole a loosely fitting plug of brass several millims. long was placed, and securely fastened with a cement of insulating material. Leading out from each plug was a wire, which was insulated for some centims. by being surrounded with a glass tube. The piece of plate glass thus prepared was placed between the poles of the magnet, precisely as a plate bearing a strip of gold would be. One of the brass plugs was placed in connexion with the inner coating of a battery of Leyden jars charged by means of a Holtz machine, the opposite plug being in connexion with the outer coating of the jars and with the earth. The other two plugs were placed in connexion with separate quadrants of a Thomson electrometer. The quadrants were both insulated from the earth. The electrometer was sufficiently sensitive to deflect the spot of light about 170 millims. for the electromo-

tive force of a Bunsen cell, or 340 millims. on reversing the connexions with such a cell. The battery of Leyden jars was charged to a potential sufficient to give a spark of 2 or 3 millims. The connexions being thus made, the position of the spot of light was observed and the magnet then operated with the purpose of discovering, if possible, any consequent change of position of the spot of light which would indicate an action of the magnet on the lines of static induction in the glass. The observation failed to establish the existence of any such action. The electrometer being in a very sensitive condition, the spot of light was rather unsteady; so that any very slight effect of the kind looked for would not have been detected, though it is probable that, if a reversal of the magnet had caused a change of four millims. in the position of the spot of light, this effect would have been apparent.

We may therefore conclude that any change of relative potential on the quadrants of the electrometer caused by reversal of the magnet was probably less than $\frac{1}{80}$ of that caused by reversing the connexions of the electrometer with a Bunsen cell, as mentioned above. If now we estimate the difference of potential between the plugs A and B, connected with the Leyden jars, to have been, as indicated by the length of the spark, equal to that which would be produced by 10,000 Bunsen cells in series, we may conclude that any difference of potential between the other plugs C and D which was caused by the action of the magnet must have been less than $\frac{1}{800000}$ of the difference of potential between A and B. We must remember, however, that any change of potential on C and D had to be extended as well over the comparatively large area of the electrometer quadrants. Professor Rowland has roughly estimated the capacity of the quadrants as twenty times that of the plugs C and D. If, therefore, these plugs had not been attached to the electrometer, any difference of potential between them due to the action of the magnet would have been twenty times as great as in the actual case; so that instead of $\frac{1}{800000}$ we have $\frac{1}{40000}$ of the difference of potential of A and B as the superior limit of the difference of potential of C and D which the magnet might possibly have produced if C and D had not been connected with the electrometer. Representing the former difference of potential by E, the latter by E′, and the strength of the magnetic field, about 4000 centim.-grm.-sec., by M, we have for this case of static induction in glass $\frac{E'}{E \times M}$, if not zero, less than $\frac{1}{160000000}$.

Turning to the analogous case of current-electricity in the various metals, and representing now by E the difference of potential of two points a centimetre apart in the direction of

the current, and by E′ the difference of potential of two points a centimetre apart in a direction at right angles to that of the current, while M has the same signification as before, we may write, as a very rough estimate for the case of iron, $\frac{E'}{E \times M} = \frac{1}{1000000}$, while for tin the value of this ratio may be as small as $\frac{1}{1600000000000}$. We may therefore conclude that the equipotential lines in the case of static induction in glass, if affected at all by the magnet, are affected much less than the equipotential lines in the case of a current in iron; but we cannot say that any such possible action in glass has been shown to be smaller than the analogous action in the case of a current in tin.

I now go on with an account of further investigation of the phenomenon actually discovered and already in some measure described in my previous article. When writing that article, it seemed to me instructive to deduce the ratio, $\frac{E}{E'}$, of the difference of potential per centim. on the longitudinal axis of the gold-leaf strip to that per centim. on the transverse axis. There were thus obtained, for the experiments made, values of $\frac{E}{E'}$, ranging from 3000 to 6500, according to the strength of the magnetic field*.

At that time I supposed that the ratio $\frac{E \times M}{E'}$ would prove to be a constant, not only for different strips of one metal, but for all conductors. Subsequent experiments showed that this was not the case; and in this article the results obtained will be expressed by the ratio $\frac{M \times V}{E'}$, where E′ has the same signification as before, while M now expresses the strength of the magnetic field in cm.-grm.-sec. units, and $V = \frac{C}{S}$† the strength of the primary current divided by the area of section of the conductor. This ratio does not prove to be the same constant

* In obtaining this latter quantity, which was called M, a serious error was made, and the value given was probably not much more than half what it should have been. This fact was mentioned in a note when the article in question was republished in Silliman's Journal for March 1880, pp. 200 & 235.

† This quantity V may be said to bear an intimate relation to the absolute velocity of the electricity; for if we were to take as the unit velocity of electricity that of a unit current flowing through a conductor of unit cross section, the velocity in any particular case would be a quantity $\frac{C}{S}$.

for different metals, but for any particular metal it seems much more nearly a constant than the ratio $\frac{\mathrm{E} \times \mathrm{M}}{\mathrm{E}'}$ given above would be.

It may seem to those who read the following pages that an unnecessary amount of study has been devoted to gold. It must be remembered, however, that many readers of my previous article were not fully convinced by the evidence there adduced that any really new principle had been discovered, thinking that the explanation of the phenomenon described was possibly to be found in some such fact as the state of mechanical strain into which the strip of gold leaf would be thrown in its endeavour to move across the lines of magnetic force, in obedience to the perfectly well-known laws of the action of magnets on conductors bearing currents. This being the case, it seemed desirable to make experiments with several strips of the same metal, and determine whether the ratio $\frac{\mathrm{M} \times \mathrm{V}}{\mathrm{E}'}$ would prove to be a constant for all. The dimensions of many of the strips used, of whatever metal, are given below; and in order that the conditions to which they were variously subjected may be more fully understood, there are given in many cases the strength of the magnetic field in absolute units and the strength of the primary current through the strip, the latter being expressed in terms of the constant k of the tangent-galvanometer used to measure it. This constant there has been no occasion to determine exactly; but it is about ·07.

It will probably be readily admitted that the results obtained cannot be accounted for without admitting substantially all that was really claimed in the previous article. Even if no such quantitative investigation had been made, however, there would still be one fact inexplicable on the theory of an accidental cause for the phenomenon under consideration. The arrows in fig. 2 show the direction of the transverse current relatively to the direct current in gold, the magnetic pole S being a south pole, *i. e.* the pole attracting the north-pointing end of a needle. This relation between the directions of the two currents and the magnetic force is the same in all of the four gold plates which have been examined in this particular. The same uniformity is observed in the four silver plates, and the three iron plates, which have been tested in the same way. With the two plates of tin which have been examined there has been a trifle of uncertainty upon this point, as the effect in this metal is at best very small; but this uncertainty is hardly sufficient to cast doubt upon the correctness of the rule

that, so far as observation has gone, the relative direction of the transverse current is always the same for any particular metal. This uniformity in so many cases could hardly be accidental.

This matter of direction is evidently one of fundamental importance. The direction was found to be the same for silver as for gold, these being the two metals first examined. Professor Rowland, however, predicted that the direction would be reversed in iron ; and experiment verified prediction. Professor Rowland's comments upon the significance of this discovery are already before the public*. It is a seemingly awkward fact that in nickel, next to iron and cobalt the most strongly magnetic substance, the direction of the transverse current is the same as in gold. This fact will be discussed further on. The conductors which have up to this date been subjected to experiment are gold, silver, iron, tin, nickel, and platinum. The direction is the same in all except iron.

The extreme irregularity in the results obtained in the early part of this course of experiments was due to various causes, only one of which is worth mentioning here. This source of error was the shape of the magnet-poles, which, being intended for the study of the magnetic rotation of polarized light, were perforated axially by a hole several millims. in diameter. With these poles the magnetic force was found to vary many per cent. in different parts of the field. These poles were subsequently replaced by solid ones ; and a sufficiently uniform field was thus secured. It will, however, be noticed that even after this change the results obtained on the same day and with the same plate often vary by several per cent. Probably quite a part of this irregularity was due to the faulty manner in which the tangent-galvanometer, which measured the strength of the primary current through the strip, was introduced. This source of error can probably be avoided in future measurements. Again, it is to be remembered that the strength of the transverse current was determined by a delicate Thomson galvanometer, an instrument far more sensitive than accurate. In using comparatively thick strips of metal there is especial liability to error from this source, as a low-resistance galvanometer must then be employed, which may easily change in sensitiveness several per cent. within an hour.

Much of the disagreement to be observed in the results obtained with different plates of the same metal is no doubt to be explained by the difficulty of determining, with any thing like accuracy, the thickness of the various strips employed. I have tried to determine approximately the thickness of the thin-

* Amer. Journ. Math. vol. ii. p. 355.

nest films used by measuring the electrical resistance; but this method, as will be seen, is exceedingly faulty. The thicker strips have been weighed before being placed on the glass; but even this method fails to determine the effective thickness accurately. Even if the specific gravity were the same for all the strips (and it probably is not), the value thus obtained for the thickness would give only the average thickness; and this is by no means the effective thickness. It will be remembered that the connexions leading to the Thomson galvanometer are placed opposite to each other, with the width of the metal strip between them. The effective thickness is the average thickness along the line joining these two side connexions. Gold foil is obtained in sheets 10 or 12 centims. square. It will be seen further on that in one case two strips cut from similar positions in the same sheet differed in average thickness about 7 per cent. This being the case, it seemed quite possible that the effective thickness of any strip, as defined above, may differ many per cent. from the mean thickness indicated by the weight.

All these sources of error being considered, the discrepancies which will be observed in the results to be given will not be surprising.

A single complete series of observations consisted of the following parts:—

1st. A determination of the extent to which the indicator of the Thomson galvanometer was affected by the direct influence of the magnet and the magnetizing current.—All that it was necessary to ascertain in this case was the change in position of the galvanometer indicator caused by reversing the current through the magnet. This usually amounted to 1 or 2 millims.; and subsequent readings of the Thomson galvanometer were, when it was necessary, corrected accordingly.

2nd. A determination of the strength of the magnetic field.—This was done by withdrawing suddenly from the field a small coil consisting of a few turns of wire and observing the effect of this action on a delicate galvanometer placed in circuit with the coil *. The galvanometer was used with a mirror and scale; and the readings actually obtained were reduced by the formula

$$\sin\frac{\Phi}{2}=\frac{n}{4r}\left(1-\frac{11}{2}\left(\frac{n}{4r}\right)^2\right),$$

where n is the actual reading and r the distance from the mirror to the scale. The constant of the galvanometer not

* Rowland, "On a Magnetic Proof Plane," Silliman's Journal, vol. x. p. 14 (1875).

being known, its sensitiveness (that is, the significance of its readings in absolute measure) was determined whenever the strength of the magnetic field was to be found. This was effected by means of an earth inductor placed in circuit with the galvanometer, and the test-coil used with the magnet. The determination of the strength of the magnetic field therefore involves two series of observations, one with the earth inductor and one with the test-coil.

3rd. A determination of the sensitiveness of the Thomson galvanometer.—This was done by sending through it a current of known strength obtained by shunting the current from a Bunsen cell, the main current being measured with a tangent-galvanometer.

4th. The main experiment.—The primary current through the metal strip measured with the tangent galvanometer just spoken of, and the effect of reversing the magnet observed on the scale of the Thomson galvanometer.

5th. Another determination of the sensitiveness of the Thomson galvanometer.—Method as described above.

6th. Another series of observations with the test-coil.

7th. Another series of observations with the earth inductor.

8th. Another determination of the direct action of the magnet on the Thomson galvanometer.

If, as was usually the case, several series were to be made with the same plate in one day for the purpose of using primary currents of various strengths, the sensitiveness of the Thomson galvanometer was tested before each main series of observations and after the last.

The mean of two values found for the sensitiveness of the Thomson galvanometer was, of course, taken to be the sensitiveness during the series of observations intervening. It was not found necessary to determine the strength of the magnetic field more than twice during a half-day's observations.

In working up these observations the following formula applies:—

$$\frac{\mathrm{M}\times\mathrm{V}}{\mathrm{E}'}=\frac{7460\,\mathrm{H}\dfrac{\sin\frac{\Phi}{2}}{\sin\frac{\Phi'}{2}}\,\dfrac{k\tan\alpha}{wt}}{\dfrac{dk\tan\Theta\,pr}{d'w}}.$$

M, V, and E′ have been already defined.

7460 = twice the integral area of the earth inductor divided by the integral area of the test-coil. Twice the simple

ratio of these two areas is taken, for the reason that the earth-inductor coils are turned through 180° when used.

H = horizontal intensity of earth's magnetism at position of earth inductor.

$\sin\frac{\Phi}{2}$ = a quantity relating to effect on the galvanometer used with test-coil, produced by withdrawing the latter from the magnetic field.

$\sin\frac{\Phi'}{2}$ = a similar quantity relating to the galvanometer and the earth inductor.

k = constant of tangent-galvanometer.

α = reading of tangent-galvanometer when measuring primary current through the metal strip.

w = effective width of metal strip.

t = effective thickness of metal strip.

d = difference in readings on the Thomson-galvanometer scale caused by reversing magnet in the main experiment.

d' = difference in readings on same scale caused by reversing current in determining sensitiveness of the Thomson galvanometer.

Θ = reading of tangent-galvanometer when measuring current used to determine sensitiveness of Thomson galvanometer.

p = proportion of the above current which passes through the Thomson galvanometer.

r = total resistance of circuit containing Thomson galvanometer during main experiment.

The above formula reduces to the form

$$\frac{\mathrm{M}\times\mathrm{V}}{\mathrm{E}'}=\frac{7460\sin\frac{\Phi}{2}\tan\alpha d'\mathrm{H}}{tdpr\tan\Theta\sin\frac{\Phi'}{2}}.$$

It will be seen that k and w have disappeared. The elimination of w is a very important fact, as this would be an exceedingly difficult quantity to determine with accuracy. As the case stands, it is not at all important to preserve the form of the metal strip after its thickness has been determined. This makes the adjustments of the side connexions (see fig. 1), leading to the Thomson galvanometer, a matter of considerable ease.

The following pages give some details of the study of the various metals examined.

Gold.

The experiments which furnished the results already published were made with gold leaf so thin as to be transparent.

In order to reduce those results to the form since adopted, it would be necessary to know the thickness of the gold strip. This thickness might be determined roughly if we knew the specific resistance of the material and the actual resistance of the strip, which is now destroyed. The latter value is known approximately; and by assuming the specific resistance to have been that of pure gold, we might arrive at a value of the ratio $\frac{M \times V}{E'}$. This value, however, would be very much larger than that obtained when thicker strips of metal are used; and facts to be hereafter mentioned make it appear quite probable that the thickness of the strip, as above arrived at, is several times smaller than the true thickness *.

Without attempting, therefore, any accurate determination of the constant of this first strip (A), I pass on to

Gold Leaf, Plate (B).

This plate also is of very thin metal; and in general I shall use the term gold *leaf* when speaking of the metal in this shape, and use the term gold *foil* to denote the strips of considerable thickness.

This second plate of gold leaf was not constructed until after several thick plates had been tried and found to give very different results from those obtained with the first thin plate in the manner described above. Thinking that some experimental error in the first measurements might account for the discrepancy, and the first plate being destroyed, I constructed the second one. In making observations with this plate I first used the high-resistance Thomson galvanometer, whereas the low-resistance instrument had been used with the thick plates. Thinking that I might in changing instruments have fallen into some error, I afterwards made another series of observations with the same plate, but using the low-resistance galvanometer. The results were (the thickness here also being estimated as above described):—

March 18,	with	high-resist.	galv.,	$\frac{M \times V}{E'}$	$=622 \times 10^{10}$
,,	,,	,,	,,	,,	$=637$,,
,, 19,	,,	low-resist.	,,	,,	$=681$,,
		Mean		,,	$=647 \times 10^{10}$

This result is about four times as large as those found with

* See also Albert v. Ettingshausen, "Bestimmung der absoluten Geschwindigkeit," &c., *Sitzungsberichte Akad. Wien*, vol. lxxxi. p. 446 (1880). He found the value of the thickness indicated by the weight in similar cases to be from four to ten times as great as that indicated by the resistance.

thicker plates. Arguing from these facts alone, it would appear that the transverse effect in thin leaf gold is relatively much smaller than the effect in strips of sensible thickness; but this is hardly a safe conclusion. Three objections to the above method of determining the thickness by means of the resistance are evident:—1st. Gold leaf so thin as to be transparent is by no means continuous, but is perforated by a multitude of small holes; so that the electricity is, as it were, obliged to wind or zigzag its way through the strip, thereby having a longer path and meeting a greater resistance than if it could pursue a direct course. 2nd. Gold leaf is an alloy about twenty-three carats fine; and the resistance of such alloys is often much larger than that of either of the pure metals. 3rd. It is difficult to secure good contact at the ends of the strip. In the plate under consideration the contact was probably very bad, and may have been many per cent. of the whole resistance of the plate as measured.

All these sources of error affect the result in the same way. To compensate, it would be necessary to diminish the resistance as measured, and then, in deducing the thickness, use a specific resistance higher than that belonging to gold. In using thin silver plates, I have in a rough way made a correction for the error due to contact-resistance; but the gold *leaf* is in several respects so unsuitable for any thing like accurate work, that it does not seem worth while to spend any more time upon it at present. In fact I would in the present article dismiss the subject of gold-*leaf* strips with a very few words, were it not the case that, in a matter of this kind, it seems proper that the public should be informed of any facts that have the slightest suspicious appearance.

The gold plates which are now to be described were of comparatively thick metal, such as is used by dentists. The metal in this shape is said to be very pure; and the thickness was so considerable as to make it possible to weigh the strips with sufficient accuracy. The determination of the thickness in this way involves the assumption that the specific gravity is that given by the tables; but the error from this source must be very much smaller than the sum of those introduced by employing the resistance method.

Gold used by dentists is classed under various heads, according to the manner of tempering. The kinds I have used are, I think, "soft" or "semicohesive," and "hard" or "cohesive." I noted the varieties, thinking that specific peculiarities might possibly appear in their behaviour. The number attached to each plate is the commercial number of the specimen, and indicates approximately the number of grains in a

sheet about 10 centims. square. The letters attached are intended to distinguish different plates constructed from gold of the same number.

Gold Foil, No. 6 A.

This strip was, I believe, of the kind called by dentists "hard," or "cohesive." To determine the thickness it was weighed before being attached to the glass. Previous experiments having shown the great variation in thickness between different parts of a sheet of gold foil, this strip was cut before weighing into nearly the same shape and size that it was to have on the glass.

The strip was in general shape a parallelogram with a projection from the middle of each of its longer sides. The use of these projections, which were much reduced in size before making the observations, has been already explained.

Length of strip when weighed	=	8·50 centims.
Width " "	=	2·14 "
Area, including projections .	=	20·5 square centims.
Weight	=	·0848 grm.

Taking the specific gravity of gold at 19·36, the value given by Ganot for "gold stamped," we find

thickness = ·000214 centim.

With this plate many series of experiments were made, yielding most of the time-results, which were very discordant, owing to various disturbing causes, some known and others perhaps unknown, to which allusion has already been made. The results obtained every day, except the last, of my working with this plate are so discordant that, in preparing them for publication, it does not seem worth while to go over again the great mass of figures involved, for the purpose of correcting any small errors of calculation. The results obtained were:—

February	20th,	$\frac{M \times V}{E'}$	$=134 \times 10^{10}$
"	"	"	$=136$ "
"	23rd,	"	$=163$ "
"	"	"	$=159$ "
"	"	"	$=166$ "
"	25th,	"	$=160$ "
"	"	"	$=149$ "
"	"	"	$=157$ "
"	27th,	"	$=152$ "
"	"	"	$=147$ "
	Mean .	"	$=152 \times 10^{10}$

Replacing now the old perforated poles of the electromagnet by solid new ones, and removing one or two other sources of error, I found:—

$$\begin{array}{rl} \text{March 5th, } \dfrac{M \times V}{E'} & = 150 \times 10^{10} \\ \text{,, ,, ,,} & = 150 \text{ ,,} \\ \text{,, ,, ,,} & = 154 \text{ ,,} \\ \hline \text{Mean .} & = 1513 \times 10^{9} \end{array}$$

The strength of the magnetic field was, as usual, determined twice on March 5th, once before and once after the other observations. The two values varied by something more than 1 per cent. The mean of the two is taken as the uniform strength for the day. The strength of the primary current sent through the gold strip was much varied for the different series of observations.

Thus we may write as corresponding to the above three values:—

Strength of field. M.	Strength of primary current. C.
6400	$k \times \tan 23^\circ\ 44'$
,,	,, ,, $42\ 14$
,,	,, ,, $49\ 28$

when k is the constant of the tangent galvanometer = ·07 nearly.

The agreement between the mean of the various results previously obtained and the mean of those found March 5th was considered satisfactory; and the next measurements were made with

Gold Foil, No. 5.

The metal in this plate was, I believe, either "soft" or "semicohesive."

Length of strip when weighed	=	8·49 centims.
Width ,, ,,	=	about 3·28 ,,
Area including projections	=	30·0 square centims.
Weight	=	·1122 grm.
Thickness	=	·000188 centim.

This strip, after being placed on the glass, was trimmed down to a width of about 2·32 centims.; and the mean thickness of this strip was no doubt quite different from the value above obtained. This strip was reduced in width, after being weighed, more than any other that has been used; and this fact may account for the discrepancy between the results

obtained with it and those obtained with the strips of No. 6, already described, and of No. 4, which is to be described next.

With No. 5 were made four series of observations, resulting thus:—

	M.	C.	$\frac{M \times V}{E'}$.
March 8th,	6400	$k \times \tan 42^\circ\ 26'$	161×10^{10}
„ „	6330	„ „ 26 2	163 „
„ 10th,	6440	„ „ 22 48	162 „
„ „	6440	„ „ 43 0	164 „
		Mean . .	$=1625 \times 10^9$

The next plate used was

Gold Foil, No. 4 (*soft*).

Length when weighed . .	= 7·64 centims.
Width „ . .	= 2·13 „
Area, including projections	= 18·46 square centims.
Weight	= ·0478 grm.
Thickness	= ·000134 centim.

With this plate four series of observations were made in one day.

The results obtained (March 12th) were

M.	C.	$\frac{M \times V}{E'}$.
6480	$k \times \tan 22^\circ\ 21'$	155×10^{10}
„	„ „ 26 25	155 „
„	„ „ 42 16	154 „
„	„ „ 28 43	154 „
	Mean . .	$=1545 \times 10^9$

Measurements had now been made with three plates of gold foil; and, considering the irregularity likely to be produced by the impossibility of determining accurately the effective thickness of the strips, the results seemed to agree satisfactorily, indicating $\frac{M \times V}{E'}$ to be a constant for this metal. If the experiments in gold had begun with these particular plates, they would probably have ended with them for the present. Owing, however, to the great discrepancy observed between these results and those obtained with the very thin plates, it seemed desirable to go further; and I therefore constructed a plate, using

Gold Foil, No. 30 A (*semicohesive?*).

Length of strip when weighed	$=5{\cdot}76$ centims.
Width ,, ,,	$=1{\cdot}085$ centim.
Area, including projections .	$=7{\cdot}36$ square centims.
Weight	$= {\cdot}161$ grm.
∴ Thickness	$= {\cdot}001129$ centim.

With this plate,

	M.	C.	$\frac{M \times V}{E'}$.
April 20th,	6520	$k \times \tan 48^\circ\ 38'$	123×10^{10}
,, 23rd,	6600	,, ,, 31 30	124 ,,
,, ,,	6600	,, ,, 40 39	128 ,,
		Mean . .	$=1250 \times 10^9$

This value is about 20 per cent. lower than the mean of those obtained with the three plates, Nos. 4, 5, and 6, previously used. The discrepancy was so great that another plate was made with a strip cut from the same sheet as No. 30 A.

Gold Foil, No. 30 B (*semicohesive?*).

Length of strip when weighed	$=5{\cdot}69$ centims.
Width ,, ,,	$=1{\cdot}08$ centim.
Area, including projections .	$=7{\cdot}33$ square centims.
Weight	$= {\cdot}149$ grm.
∴ Thickness	$= {\cdot}00105$ centim.

It will be seen that the strips A and B, cut from similar positions in the same sheet of metal, differ about 7 per cent. in mean thickness. The importance of this fact has already been pointed out. The difference in thickness thus found was so great that I at first supposed a mistake must have been made in weighing the first strip, thereby giving too large a value for the weight. I therefore removed the strip from the glass plate and weighed it again. The result confirmed the original value obtained.

With the new plate, No. 30 B, I found:—

	M.	C.	$\frac{M \times V}{E'}$.
April 26th,	6760	$k \times \tan 68^\circ\ 0'$	139×10^{10}
,,	,,	,, ,, 39 26	141 ,,
		Mean . .	$=1400 \times 10^9$

This value is much nearer those obtained with the plates 4, 5, and 6; but even now there is a discrepancy of 8 or 10 per cent. Without discussing this matter any further at present, I pass on to tell what has been observed with

Silver.

Measurements have been made with four separate plates of this metal. The thickness of the strip was estimated in one case by weighing, in the three others by measuring the electrical resistance. I will give first the results obtained with the thick strip.

Silver-foil, No. 10.

Length of strip when weighed	$=7{\cdot}98$ centims.
Width „ „	$=1{\cdot}07$ centim.
Area, including projections .	$=9{\cdot}23$ square centims.
Weight	$=\cdot0474$ grm.
∴ Thickness (taking sp. gr. to be 10·47)	$=\cdot000491$ centim.

With this plate,

	M.	C.	$\frac{M\times V}{E'}$.
April 21st,	6580	$k\times\tan 49^\circ\ 17'$	114×10^{10}
„	„	„ „ 32 20	118 „
		Mean . .	$=1160\times10^{9}$

Two of the other plates were prepared, not by fastening silver-leaf to glass with shellac, but by depositing from a solution the silver directly upon the glass. The process made use of for this purpose was Böttger's, as detailed in Silliman's Journal for 1867. The two plates were cut from the same piece of glass, after coating.

Silver Film A.

Length between the contact blocks	$=6{\cdot}05$ centims.
Width	$=2{\cdot}46$ centims.
Electrical resistance, as measured .	$=1{\cdot}45$ ohm.

Knowing that the contact-resistance must be quite a part of this value, I endeavoured to determine its amount roughly in the following manner:—Having obtained the above value, 1·45 ohm, and measured the distance between the blocks, I shortened the strip by placing the blocks nearer together, then measured the length and again determined the resistance of the whole. This process was repeated, thus giving three values of the resistance, corresponding to the three lengths of the strip employed. From these values the contact-resistance is readily determined, though of course very roughly. It appeared to be equal to the resistance of about 2·7 centims. of the strip itself; and therefore, in estimating the thickness of the strip from the electrical resistance, the effective length of the

strip was taken to be not 6·05, but 8·8 centims. Assuming the specific resistance of the silver in this plate to be ·00000165 ohm (the value given by Jenkin for "hard-drawn" silver), we obtain as the thickness of the strip ·00000407 centim. It will be shown below that this value is probably very much too small; but I will for the moment give the results obtained on the basis of this estimation of the thickness.

Passing over a result obtained at quite an early period of the experiments, and which there are excellent reasons for rejecting, we have

	M.	C.	$\frac{M \times V}{E'}$.
Jan. 30th,	7120	$k \times \tan 43^\circ\ 33'$	487×10^{10}
,,	,,	,, ,, 19 32	499×10^{10}
		Mean . .	$=493 \times 10^{10}$

The discrepancy between this result and that obtained with the thicker strip of silver was so great that I determined to try

Silver Film B.

I have assumed the thickness of B to be the same as that of A. The other dimensions are about the same; and the result is

	M.	C.	$\frac{M \times V}{E'}$.
May 4th,	6640	$k \times \tan 47^\circ\ 30'$	491×10^{10}

The agreement of this result with the mean of those just preceding is entirely satisfactory, and the discrepancy above mentioned as existing between the results with plates of different kinds is confirmed. This disagreement was so large as to be difficult to account for, without the hypothesis of a specific difference exhibited by different forms of the same metal, under the conditions of the experiment. To be sure, the method of estimating the thickness from the electrical resistance was open to suspicion. Among other probable sources of error, there was the possibility of having assumed a wrong value for the specific resistance of the silver in this condition. It did not appear to me probable that an error of about 400 per cent. could be accounted for in this way; but it seemed worth while to attempt a determination of the thickness of the films by another method.

Plate A was taken and cleaned with alcohol to remove the particles of cement adhering to the glass and metal. The area of the silver film was roughly determined; and the plate was dried and, when cool, carefully weighed. The silver was then removed by dissolving in nitric acid, after which the glass was

again dried and weighed. In addition to this the solution of silver was filtered and treated with hydrochloric acid. The precipitate was filtered off, and the silver reduced by burning with the filter-paper. The amount of silver on the glass was thus estimated in two ways. According to the weight lost by the plate the amount of silver appeared to be 4·3 mgrs.; while the amount obtained by the chemical process was only about 2·5 mgrs. There are good reasons for thinking the former value too great, and some reasons for thinking the latter too small. Giving the latter double weight in taking the mean, we get $\frac{4{\cdot}3+2\times2{\cdot}5}{3}=3{\cdot}1$ mgrs. for the amount of silver in the film. The area covered by this on the glass was about 20 square centims. Taking the specific gravity of silver to be 10·5, we get for the thickness of the film,

$$t=\frac{{\cdot}0031}{20\times10{\cdot}5}={\cdot}0000148 \text{ centim.}$$

This value is more than 3·6 times as large as that obtained by the resistance method. In order to make perfect accord between the results obtained with the two kinds of silver plates, the thickness would need to be rather more than four times as great as that obtained by the resistance method; but, considering all the difficulties of the case, it seems to me that the large discrepancy still existing is within the limits of experimental error. In presenting the results of all the experiments in tabular form further on, I shall give the results obtained with these silver films as calculated on the basis of the larger value (*i. e.* ·0000148 centim.), found for the thickness.

Mention is made above of a fourth plate of silver. This was also of a very thin film; but the silver was fastened to the glass with shellac instead of being deposited from a solution. The silver was in the same state as that of the thickest plate; and the results of measurements with it accord sufficiently well with those obtained with that plate. As the resistance method was employed in estimating the thickness, it does not seem worth while to publish the results obtained.

Iron.

Measurements have been made with three separate plates of iron. The first two plates were made early in the research; and the quantitative results, like all others obtained at that time, are hardly reliable enough to be worth publishing.

The dimensions of the third strip were as follows:—

Length, as weighed	$=5{\cdot}68$ centims.
Width "	$=1{\cdot}08$ centim.
Area, including projections . . .	$=7{\cdot}15$ square centims.
Weight	$=\ {\cdot}193$ grm.
∴ Thickness (taking sp. gr. $=7{\cdot}71$)	$=\ {\cdot}00347$ centim.*

With this plate the following results were obtained:—

	M.	C.	$\frac{M \times V}{E'}$.†
April 29th,	6680	$k \times \tan 38^\circ\ 37'$	-127×19^9
"	"	" " 49 13	-130 "
		Mean .	$=-1285 \times 10^8$

Platinum.

One strip of this metal has been used.

Length, as weighed	$=6{\cdot}32$ centims.
Width, "	$=1{\cdot}078$ centim.
Area, including projections . . .	$=7{\cdot}57$ square centims.
Weight	$=\ {\cdot}457$ grm.
∴ Thickness (taking sp. gr. $=22{\cdot}1$)	$=\ {\cdot}00274$ centim.

With this strip only one series of observations was made; and that was rather a hasty one. I found:—

	M.	C.	$\frac{M \times V}{E'}$.
April 28th,	6830	$k \times \tan 66^\circ\ 2'$	417×10^{10}

Nickel.

There was some difficulty in obtaining a strip of this metal of proper shape for the experiment. The piece used was obtained by stripping off the nickel plating from a piece of brass, upon which the deposit had been purposely laid in such a manner as to make it easy to remove. The strip thus obtained was narrow and irregular in shape; and its thickness cannot readily be determined at present; so that I do not attempt to

* The plates of very thin rolled iron used were furnished me by Prof. Rowland, who is indebted for a supply of the same to the courtesy of Prof. Langley, of Allegheny Observatory.

† It is evident that the values of this ratio thus obtained for iron are to some extent, perhaps to a great extent, fictitious; for of course the strength of the magnetic field within the iron plate itself is the effective strength in the experiment, and this is probably very different from the value of M as determined by means of the test-coil. It seems best, however, for the present to employ this latter value of M, which must bear an intimate relation to the true value, and which has the great advantage of being easily determinable. Nickel has hardly been examined quantitatively as yet; and platinum is not sufficiently magnetic to present any difficulty of this sort.

give numerical results for this metal. The main object in using it was to determine the direction of the new effect therein, nickel being, next to iron and cobalt, the most strongly magnetic substance. As already stated, this direction was found to be opposite to that in iron. The action in nickel, though not really measured, was seen to be very decided, and may possibly prove to be as strong as that in iron.

Tin.

The action in this metal is very small, and has not been measured with any accuracy. Its magnitude may be one thirtieth of that of the action in gold.

No other conductors have been tested in such a manner as to warrant an expectation of detecting an action.

In the following table the results obtained with the different metals are brought together. Those obtained with very thin strips will be marked thus (?), for reasons which must be evident to any one who has read the preceding pages:—

Metal plate.	M.	C.	$\frac{M \times V}{E'}$.	
Gold, No. 6 ["hard"]	...	......	152×10^{10}	1515×10^{9}
,, ,, ,, ...	6400	$k \times \tan 23^{\circ}\ 44'$	150×10^{10}	
,, ,, ...	6400	,, ,, 42 14	150×10^{10}	
,, ,, ...	6400	,, ,, 49 28	154×10^{10}	
,, No. 5 [soft or semicohes.]	6400	,, ,, 42 26	161×10^{10}	1625×10^{9}
,, ,, ...	6330	,, ,, 26 2	163×10^{10}	
,, ,, ...	6440	,, ,, 22 48	162×10^{10}	
,, ,, ...	6440	,, ,, 43 0	164×10^{10}	
,, No. 4 ["soft"]	6480	,, ,, 22 21	155×10^{10}	1545×10^{9}
,, ,, ...	6480	,, ,, 26 25	155×10^{10}	
,, ,, ...	6480	,, ,, 42 16	154×10^{10}	
,, ,, ...	6480	,, ,, 28 43	154×10^{10}	
,, No. 30 A [semicohes.?]	6520	,, ,, 48 38	123×10^{10}	1250×10^{9}
,, ,, ,, ...	6600	,, ,, 31 30	124×10^{10}	
,, ,, ,, ...	6600	,, ,, 40 39	128×10^{10}	
,, ,, B [semicohes.?]	6760	,, ,, 68 0	139×10^{10}	1400×10^{9}
,, ,, ,, ...	6760	,, ,, 39 26	141×10^{10}	
Silver, No. 10	6580	,, ,, 49 17	114×10^{10}	1160×10^{9}
,, ,, ...	6580	,, ,, 32 20	118×10^{10}	
,, [deposited] A	7120	,, ,, 43 33	134×10^{10}	1355×10^{9} ?
,, ,, ,, ...	7120	,, ,, 19 32	137×10^{10}	
,, ,, B	6640	,, ,, 47 39	......	1350×10^{9} ?
Iron, C	6680	,, ,, 38 37	-127×10^{9}	1285×10^{8}
,, ,, ...	6680	,, ,, 46 13	-130	
Platinum	6830	,, ,, 66 2	......	4170×10^{9}

Nickel—effect large, possibly as strong as in iron.
Tin—effect probably much smaller than in platinum.

This table enables us to arrange the metals so far examined, excepting nickel, in order, with respect to the magnitude of the action observed in them. Opposite each metal in the following list is placed a number representative of this magnitude. In the case of gold this number is a quantity inversely pro-

portional to the mean of the results obtained with the five different plates named above. In finding the corresponding number for silver, I have, for obvious reasons, used only the result obtained with the plate of No. 10. The representative number given for tin has been very roughly estimated, and may be one or two hundred per cent. larger or smaller than the true number. All the numbers given must, of course, be taken as at best only rough approximations to the true representative numbers.

We find, then,

Iron	−78
Silver	8·6
Gold	6·8
Platinum	2·4
Tin	·2 (?)

This arrangement is made on the basis of defining the magnitude of the action studied as a quantity inversely proportional to $\frac{M \times V}{E'}$. If, on the other hand, we were to define the same as inversely proportional to $\frac{M \times E}{E'}$ rather, E being the difference of potential of two points a centimetre apart on the longitudinal axis of the metal strip, the representative numbers would be relatively changed. The representative numbers on this new basis may be found by simply dividing each of the representative numbers given above by a quantity proportional to the specific electrical resistance of the metal to which the number is attached.

We thus obtain :—

Iron	−80
Silver	57
Gold	32
Platinum	2·6
Tin	·15 (?)

It will be observed that the order of arrangement remains unchanged.

Platinum and tin are carried still further from gold and silver than before; so that the range of the representative numbers is increased. It is plain, therefore, that by this second arrangement no progress has been made toward finding a constant representative quantity for all the metals. In dealing with the results obtained with different metals, it seems to be of little importance whether we take as our basis $\frac{M \times V}{E'}$ or $\frac{M \times E}{E'}$. When, however, we have to do with dif-

ferent plates of the same metal, we see from the experiments on both gold and silver that the basis $\frac{M \times V}{E'}$ is by far the better one. We may sum up the matter by saying that, according to present appearances,

1st, there is no constant representative quantity for all metals;

2nd, the basis $\frac{M \times E}{E'}$ does not give a constant representative quantity for different plates of the same metal;

3rd, the basis $\frac{M \times V}{E'}$ gives for different plates of the same metal a representative quantity which is approximately a constant.

It is evident, upon consideration, that this ratio $\frac{M \times V}{E'}$ could not be expected to give the same result for all metals. We get the quantity V by dividing the nominal cross section of our conductor by the strength of the current. We must, however, think of a metal as not strictly continuous, but consisting of metallic particles more or less compactly aggregated in the space occupied by the body as a whole. Evidently, therefore, the cross section effective in conduction would vary in different conductors of the same nominal cross section. It may therefore be found that different specimens of the same metal, but of different densities, will give quite different values for $\frac{M \times V}{E'}$.

Of course the magnitude of the new action in the different metals may be considered in connexion wtth various other physical properties of the metal beside the specific electrical resistance. One might, for instance, expect to find some striking relation by comparing in this connexion the known magnetic or diamagnetic properties of the metals. It is indeed to be observed that the most strongly magnetic substance, iron, does show the new action in a more marked degree than the other metals; and possibly nickel will come next in the list. Here the clue is entirely lost, however; for the relative magnitude of the action in gold, silver, &c. is entirely out of proportion to the magnetic capacities of these metals.

On the whole, we cannot be sure that any relation has yet been detected between the magnitude of the new action in the various metals and any known physical property of these metals. It is of course possible, however, that, when more data shall have been obtained, analogies and relations at present unsuspected will appear. It can hardly be doubted that

the action we have been considering, placing at our command, as it does, a new point of view from which to study the interior workings of the substances examined, is destined to teach us a good deal in regard to the molecular structure of bodies, while helping us toward an understanding of the physical nature of electricity and magnetism.

We return now to the remarkable anomaly presented by the direction of the action in iron. That the direction in this metal, a magnetic substance, should be different from that in gold, a diamagnetic substance, is remarkable, but not perhaps surprising. We find, however, that nickel and platinum, both magnetic substances, resemble in the particular above mentioned, not iron, but gold and the other diamagnetic substances. This fact has to be taken into account in endeavouring to apply the newly discovered action to explain the magnetic rotation of the plane of polarization in accordance with the principles of Maxwell's electromagnetic theory of light. Professor Rowland, therefore, in view of this difference of behaviour of iron and nickel with respect to electricity, was very desirous to know whether these two metals would manifest a similar disagreement in their action upon light. I have therefore, at his suggestion, repeated Kerr's experiment on the rotation of the plane of polarization of light by reflection from the pole of a magnet, using nickel for the latter instead of iron. The reflecting surface used was the nickel plating on one of the disks of Professor Rowland's absolute electrometer. This disk, for the purpose of the experiment, was placed between the poles of the electromagnet. The action upon the plane of polarization, though apparently much weaker than in iron, has, in the plate used, unmistakably the same direction. This nickel plating, however, was executed in Germany; and Professor Rowland thinks that, as the nickel of that country is very impure, this specimen may possibly contain iron enough to mask the true action of the nickel.

I have already spoken of the fact that, when a strongly magnetic substance is experimented upon, complications are introduced by the influence of the induced magnetism, which affects the condition of the magnetic field through which the current flows, making the value of M different from that determined by means of the test-coil. It does not seem probable that in this fact can be found an explanation of the anomalous behaviour of iron; but there is no doubt that an interesting research is here suggested. For instance, it might be profitable to subject to experiment a thin plate of hard steel, and determine to what extent the permanent magnetization induced therein by the electromagnet would be accompanied by a per-

manent change in the equipotential lines after the electromagnet had ceased to act.

It is perhaps idle to speculate as to the exact manner in which the action between the magnet and the current takes place in any of the preceding experiments; but it may be worth while to remark a seeming analogy, somewhat strained perhaps, between this action and a familiar mechanical phenomenon, the theory of which has of late attracted considerable attention. It is well known that a base-ball projected swiftly through the air, and having at the same time a rapid motion of rotation about its vertical axis, does not throughout its course continue in the original vertical plane of its motion, but follows a path curving sensibly to one side. Imagine now an electrical current to consist of particles analogous to the base-ball moving through a metallic conductor, the electrical resistance of which will correspond to the mechanical resistance offered by the air. Suppose, further, the particles of electricity, on coming within the influence of the magnet, to acquire a motion of rotation about an axis parallel to the axis of the magnet *. Under all these supposed conditions we might perhaps expect to find the action which is actually detected. To account for the reversal of the action in iron we might suppose the particles of electricity to acquire in this metal a rotation about the same axis as in the other metals, but in the opposite direction. Even after all these generous concessions in favour of our hypothesis, however, it fails to account for the behaviour of nickel as different from that of iron. The analogy, such as it is, which has been pointed out, is perhaps curious rather than significant.

Historical.

I am not aware that investigators during the first part of the century made any attempt to discover the phenomenon which has been the subject of the observations described in the preceding article. Wiedemann †, however, mentions two investigators who have at different times given the subject their attention. The first of these in point of time was Feilitzsch ‡. He made use of two flat spirals of wire, through each of which an electric current was made to pass. These currents, passing

* Maxwell (‘Electricity and Magnetism,’ vol. ii. p. 416) says:—“I think we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field, that this rotation is performed by a great number of very small portions of matter, each rotating on its own axis, this axis being parallel to the direction of the magnetic force,” &c.

† *Galvanismus*, vol. ii. p. 174.

‡ *Berichte der Naturforscher in Karlsruhe*, 1858, p. 151 &c.

in opposite directions through the coils of a differential galvanometer, were so adjusted that their combined action produced no effect upon the needle. A third spiral, similar to the others and itself bearing a current, was now brought near one of these, and the galvanometer was observed. No permanent deflection of the needle was detected ; and therefore no permanent action of one current on the other was discovered. I have not had access to the original article, and cannot say what the author's theory of the experiment may have been. The method of attacking the problem seems, however, to have been similar in principle to that which I at first adopted, viz. an endeavour to increase the resistance experienced by an electrical current by diverting it from its normal course through the conductor.

Another research in this direction mentioned by Wiedemann was that of Mach *. This investigator covered a circular disk of silver leaf with wax, and applied the poles of a battery to points diametrically opposite each other on the circumference of the disk. The silver leaf becoming heated by the current, the wax began to melt, and melted most rapidly where the current was strongest, thus roughly showing the distribution of the stream. The plate, still bearing the current, was now subjected to the action of an electromagnet; but no change could be detected in the behaviour of the melting wax, the current remaining apparently unchanged in its course through the disk. This experiment, therefore, like the preceding, was negative in its indications.

A recent number of the *Beiblätter zu Wiedemann's Annalen* mentions, in connexion with the researches of Feilitzsch and Mach, another by Gore †. The latter took a wire bifurcated throughout a part of its length, and passed through it a current sufficiently strong to raise both branches to a white heat. He then endeavoured, by means of a magnet, to divert the current somewhat from one branch of the wire and draw into the other branch more than its normal share. It was thought that an unequal division of the current might show itself by a change in the appearance of the white-hot branches. No change of this kind could be detected ; and the investigator therefore concluded that the action known to take place between conductors bearing currents was not an action between the electric currents as such. Gore expressly states that he undertook this experiment not knowing that any previous investigations with the same aim had ever been made.

* Carl's *Repertorium*, vol. vi. p. 10 (1870).

† "On the Attraction of Magnets and Electric Currents," Phil. Mag. [IV.] vol. xlviii. p. 393 (1874).

On the same page of the *Galvanismus* which treats of the research of Mach, as mentioned above, Wiedemann describes, as a means of showing that no action takes place between permanent electric currents as such, almost the exact arrangement of apparatus with which the discovery was finally made. Who first used this apparatus for this purpose I cannot say, unless it may have been Wiedemann himself. The same plan was hit upon by Professor Rowland * (quite independently, I believe); and he experimented to some extent in this direction about the year 1876. The same arrangement was finally adopted by me after another method of attacking the problem had been unsuccessfully tried.

I desire to express my sense of obligation to the Professors and students of the Physical Department of the Johns Hopkins University for the generous assistance which they have rendered me during the progress of this research.

XXXIX. *On Magneto-Electric Induction.*—Part II. *Conductivity of Liquids. By* FREDERICK GUTHRIE *and* C. V. BOYS, *Assoc. R. Sch. Mines*†.

[Plate VIII.]

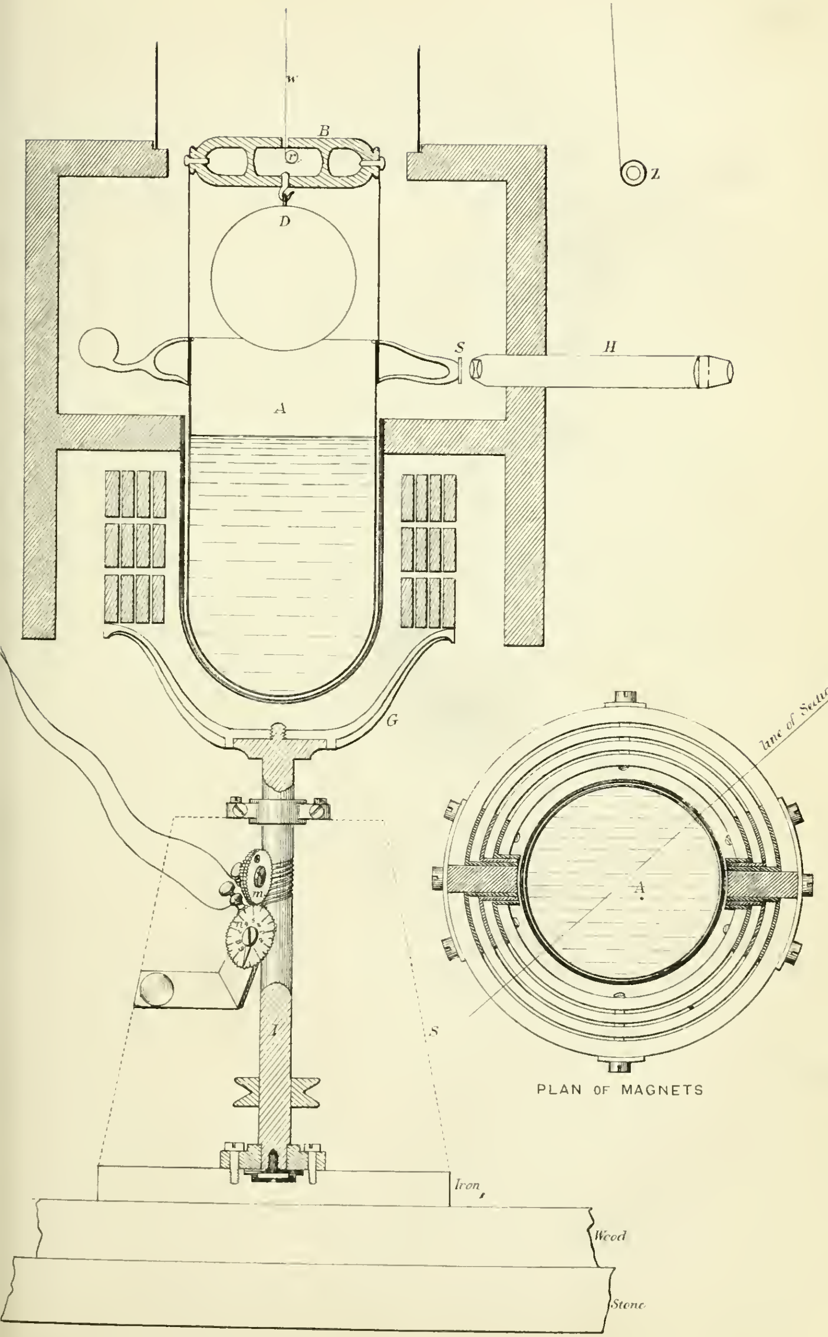
IN a previous communication ‡ we showed experimentally that, other things being equal, a conductor in a moving magnetic field is urged to move by a force which varies as the product of the conductivity into the relative speed; so that by observing the torsion of a wire supporting successively a variety of conductors of the same form and size in a revolving magnetic field, a measure of their relative conductivity may be obtained. In the case of most metals, this method of determining conductivity cannot be compared, at least for convenience, with the usual one with the bridge, galvanometer, &c.; but with less-perfect conductors, with such as cannot be drawn into wire, and especially with electrolytes, our method seemed very promising; for, whatever be the actual course of the electrical currents induced in the liquid, they are closed, no electrodes are present, there is no electrolysis, and there is no polarization.

The preliminary experiments, performed with the apparatus described in our last paper, clearly showed that with suitable means a measurable and even a large effect might be produced.

* Amer. Journ. of Math. vol. ii. p. 289.

† Communicated by the Physical Society, having been read at the Meeting on June 26th.

‡ Proceedings of the Physical Society of London, vol. iii. part iii. p. 127; Phil. Mag. December 1879, vol. viii. p. 449.


w
B
r
D
Z
S
H
A
G
m
I
S
line of Section
A
PLAN OF MAGNETS
Iron
Wood
Stone

Accordingly the experiments were continued with the very powerful and delicate apparatus shown in Plate VIII. One litre of liquid is put into the vessel A, which consists of a glass shade, the open end of which has been roughened with emery, and a strip of strong brown paper attached to it with paste. Two vertical strips of ebonite are fastened to the paper ring with shellac, and bound round with cotton, and the whole well varnished with shellac varnish. These strips are fastened by ivory pins to the boxwood beam B; and to the centre of this beam the torsion wire w is made fast by being wound three times round the little reel r and the end secured by a needle-point. The torsion wire passes over a zinc wheel about 13 feet above the beam B, and down again to the same level, where it passes round the zinc screw z; so that by turning this screw the vessel A can always be brought to the same level, whatever be the specific gravity of the liquid within it. To the top of this vessel are attached two glass arms, one on each side, one carrying a photographically-reduced scale, s, on glass, and the other a counterpoise. The vessel A is entirely enclosed in a screen shown dark in the Plate. The thick portions of the screen are of wood; but the thin cylindrical part immediately round A is of thin ebonite, and the hemispherical part below A is of glass. This screen prevents the whirlwind of air caused by the revolving magnets from interfering with the motion of the vessel A. It also supports a microscope, H, provided with cross-wires, which is used to read the position of the scale S. The magnet consists of 24 semicircular bars put together as shown, so as to form a ring, the similar poles of the different bars being kept from touching one another by the fourteen pieces of iron shown dark in the plan. The magnet is $7\frac{1}{2}$ in. in external diameter, and weighs about 18 lbs. The magnetic field within it is remarkably uniform; iron-filings on glass take the form shown by the streak lines in the plan. The magnets are attached by four brass plates to the gun-metal wheel, G, which screws against an accurately turned shoulder on the vertical steel shaft, I. This shaft runs in cylindrical split gun-metal bearings; but its weight is taken by a slightly convex hardened steel round-headed pin let into the shaft and resting on a plate of quartz. The framework which carries the bearings consists of five triangles of cast iron screwed together in the form of a distorted octahedron, a form which gives great rigidity; it is omitted in the Plate to avoid complication; but its general outline is shown by dotted lines s. The lower triangle of the framework is screwed to a thick slab of mahogany, which is clamped to the stone mantlepiece of the room; and one

hundredweight of iron stands on the wood to increase its inertia and make the whole thing more steady. Near the top of the shaft I a fine screw is cut, which gears with the worm-wheel m of 100 teeth. This wheel has also cut on its edge a screw of the same pitch as that on the shaft I; so that its edge is divided into a series of square pyramids, and it gears as a screw into the worm-wheel n of 100 teeth: n therefore turns round once for 100 turns of m or for 10,000 turns of I. At every turn m makes an electric contact and sounds a bell; n has its face divided and numbered. Therefore by observing occasionally the exact instant at which the bell sounds, and then the reading on n, a continuous record of the speed can be kept, at the same time that s is being observed with the microscope. A pulley-wheel on I is connected by a catgut band with a small steam-engine driven by steam from the boiler which supplies the building. The steam-pressure varies so much that the resulting changes in speed in the engine have caused the greatest inconvenience, sometimes a whole day being lost. Some form of absolute governor will have to be devised in order to avoid great loss of time.

On the lower side of the beam B is a small glass hook, from which may be hung a disk of brass D, the use of which will be explained below. The dimensions of most of the parts of the apparatus may be found from the Plate, which is drawn to the scale of almost exactly $\frac{1}{4}$. The torsion-thread is of hard drawn steel wire about 13 feet long and 0·007 in. in diameter. The space in which the cylindrical part of the screen is placed between the poles of the magnet and the outside of the cell is only about $\frac{3}{16}$ in. wide, so as to get the greatest possible effect from the magnets. When the machine was put together the magnets were found to be slightly out of balance; so a plate of brass full of tapped holes was fixed to the light side, insufficient in weight to restore the balance, and then small screws of brass were screwed into the holes until the balance was perfect. By this means the magnets were made to run so smoothly that at 3000 turns a minute scarcely any tremor could be felt; but always on passing a certain critical speed (about 1300 turns a minute), at which the period of vibration of the whole machine and mantlepiece was probably equal to the time of rotation of the magnets, the machine set up a vibration, gentle at first, but increasing with the speed, which seemed at first as if high speeds could never be attained. But it was found that if, as soon as this effect took place, one of the screws of the upper bearing was loosened the oscillation ceased suddenly, and then the screw could be tightened again, and the speed increased to any extent without the

slightest sign of vibration reoccuring. In fact the magnets spun like a great top, though the screwed-up bearing permitted no shake of the shaft within it. On passing the critical speed when stopping, the oscillation set up is hardly noticeable.

Before beginning the experiments it was necessary to determine what effect, if any, the magnets had on the vessel A. For this purpose it was filled with distilled water, and the magnets made to run at speeds high and low; but not the slightest movement of the scale could be detected by the microscope so long as the magnets were revolving. Had it moved the hundredth part of a millimetre, that must have been seen; but when the magnets were stopped the scale was found to have moved; and as the magnets were turned round to successive positions through 360°, the scale went through a complete oscillation of 18 divisions. These two preliminary experiments showed that, though the cell was affected by placing the magnet in different positions (an effect probably due to the three turns of the torsion-wire round the reel in the beam having a directive action), yet electrically the cell was all that could be desired, and any torsion observed during the rapid revolution of the magnets must be due to the conductivity of the liquid in the cell. No currents could be induced in the torsion-wire round the reel, for they formed an open circuit; but even if closed, the reaction of the induced currents on the magnets would be inappreciable, owing to the small diameter of the coil and its great distance above the magnets. It is also clear that the magnets could have no mechanical action on the cell due to air-currents, vibration, &c. It should be mentioned here that the screen was fixed to a panel screwed to the wall, so as to be quite independent of any thing connected with the magnets; also the torsion-wire was hung from a bracket fixed to the wall under the cornice of the room. This separation of the supports of the various parts is to ensure there being no mechanical action causing torsion due to vibration &c., an effect observed by Snow Harris in certain of his experiments.

Having now made clear the construction of the apparatus, it will be well to explain next the principle of its action, and then to describe the method of working with it, giving at the same time the few results that have been obtained so far.

When a conductor is in a moving magnetic field it is urged in the direction of motion by a force which varies directly as the relative movement, directly as the intensity of the field, and directly as the conductivity of the conductor, *i. e.* if the form of the conductor is always the same. In the present

case the form of the conductor is necessarily always the same and its situation in the magnet is so too; for its height is adjusted by the screw z till a horizontal line on the scale s is seen on the cross-wires in the microscope. The strength of the magnet is nearly constant; and its variations are allowed for by a method which will be described below: the speed of the magnets can be accurately measured by the counter; and therefore the torsion of the wire w gives an exact comparative measure of the conductivity of the liquid in the vessel A, provided that the torsion of the wire is uniform and that the movement of the liquid in the cell has no influence on the result. As the wire used cannot be made to carry 7 lbs., but has in the course of the experiments to support the cell together with a litre of liquid of any specific gravity between those of water and oil of vitriol, its torsion is any thing but constant from day to day; but its variations, due to these great changes in the load and to slight changes in temperature, are accurately allowed for at the same time that the change in the strength of the magnet is taken into account; and therefore, if movement of liquid in the cell does not vitiate the results, this method of comparing the conductivity of electrolytes is free from error.

To ascertain what effect movement of liquid in the cell has on the result, it will be necessary to examine more closely how the torsion of the wire is produced. When the magnets are revolving, currents are induced in the liquid in a direction to oppose the motion; so the liquid is pushed round in the direction in which the magnets are revolving by a force which is directly proportional to the difference in speed between the liquid and the magnets. If there were no friction of any kind tending to resist the motion of the liquid, it would in time attain a velocity equal to that of the magnets; for so long as it was revolving more slowly than the magnets there would be a force urging it on. But there is friction between the outer layer of the liquid and the vessel, and between each successive cylindrical layer and the one outside it; and therefore the liquid never attains any great velocity at all. But each elementary cylindrical layer soon reaches that speed at which the force urging it forward is equal to the friction between it and the layer outside; and therefore the torsion measured is exactly the same that would be obtained if it were possible to integrate the force from the centre to the edge of the vessel. If this reasoning is correct, the liquid should be revolving fastest at the centre and more slowly outside; and this has been proved to be the case by a very striking experiment. A solution of indigo was made of such

a specific gravity that a drop of it would just sink in the liquid used; and when the magnets were revolving at a great speed a series of drops were delivered at different distances from the centre. They all fell in spiral lines, each one revolving more quickly than the one outside it. Then, on reversing the engine and repeating the experiment the spirals were found to be going the other way, but, in each case, in the same direction as the magnets. The average speed of the liquid (30 p. c. H_2SO_4, 70 p. c. H_2O) was about one turn in ten minutes. It is now possible to see to what extent movement of liquid in the cell affects the result. As soon as the rotation of the liquid has become constant, the force urging it forward is equal to that due to friction retarding it; the torsion of the wire therefore is an exact measure of the force on the *moving* liquid. But the force is directly proportional to the relative speed, and not to the actual speed, of the magnets; and we have seen that the liquid does not revolve more than once for 20,000 turns of the magnets; and therefore the error made on the supposition that the liquid is at rest is not more than the 20,000th part of the whole result— a quantity altogether inappreciable, for neither the speed nor the torsion can be measured with such accuracy.

The apparatus described was finished and the experiments begun on April 5, 1880; but it was immediately found that the behaviour of the wire was such that no results of any value could be obtained; for the zero on the scale (that is, the position without torsion) was constantly changing, so much so that sometimes without any apparent cause the scale would move ten divisions in a few hours. As 11·4 divisions correspond to an angular movement of one degree, and as the greatest torsion ever observed caused a movement of only 45 divisions, it seemed that some other wire would have to be used. But, again, steel was the only metal that could be used, as a weaker metal must have been much thicker, and the torsion of a thicker wire would have been too great. Weights were hung on the wire and twisted several times round and then left, to see if the wire would improve by such treatment, but without effect. Then some other wire was sent for, and the machine left for six weeks. During this time the old wire became a little rusty; and when the experiments were to have been continued with the new wire, the old wire was tried once more and was found greatly improved, possibly from the removal of the hard skin by rust. During the course of one experiment, lasting over an hour, the zero had not shifted the tenth part of a division. The next day the same acid was examined again; and the two

results differed by only 1·3 p. c. This result seemed very satisfactory, especially as possible changes in the strength of the magnet or in the torsion of the wire were not considered; and therefore we determined that the experiments should be continued with the old wire. So it was well rubbed with sperm oil to stop the rust from increasing in quantity and destroying the wire.

The method of carrying out the experiments must be described next. It has already been shown that the magnets when at rest have a directive action on the cell, but when in motion their action on the cell itself is nothing; yet if a conducting liquid is in it the cell is turned round. Also the zero of the scale can only be determined after the liquid is put in; and therefore there is no possibility of observing the zero directly. But as the torsional effect is proportional to the speed, by running the magnets first at a low and then at a high speed and taking the difference in the speeds (S) and also the difference in the readings (T), and dividing S by T, a number is obtained which is a measure of the resistance of the liquid in the cell. To diminish as much as possible all chances of error, every liquid was examined at four speeds—two with the magnets turning in one direction (+), and two in the other direction (−); and the results were taken in pairs, in the order of observation, to avoid errors due to such slow changes in the zero as were still liable to take place. If the strength of the magnets and the torsion of the wire were both constant, nothing more would be required; but as both are liable to change, some standard of comparison by which the value of an observed deflection can be measured is necessary. The most obvious plan is occasionally to examine some given liquid of good conductivity, *e. g.* 30 p. c. sulphuric acid, and consider variations in the result due to changes in the machine, and correct the measures obtained for other liquids accordingly. But this plan would involve a great waste of time, as from two to three hours are required for the examination of a liquid; and it would not be trustworthy, for it cannot be supposed that the torsion of a wire is the same when stretched to 50 p. c. and to 80 p. c. of its breaking strain; and therefore the standard of comparison must be applied at every experiment when the liquid under observation is in the cell. Accordingly a disk of thin sheet brass, D, three inches in diameter, was used as a comparison plate. It is hung to the glass hook below the beam B, immediately after the observation at the fourth speed has been made, without stopping the engine or touching any other part of the apparatus, and the increase in torsion noted. This increase in the torsion when corrected for speed should be constant; and the slight

variations in the increase are due to changes in the magnets, or the wire, or both, the effect of which may therefore be allowed for accurately. It is true that possible changes in the distribution of the magnetism cannot be taken into account; so that, if the upper magnets changed more or less than the lower ones, the comparison plate would indicate too great or too small a change; but no error of any importance is likely to result from such a cause. Also changes in the conductivity of the brass due to temperature have not been considered, though they can be at any time. After the observed conductivity of a liquid has been corrected by means of this comparison plate, it is independent of changes in the magnets or the wire, and so all results obtained during any length of time are directly comparable. Of course, after any great length of time an experiment would be made on 30-p.-c. sulphuric acid to serve merely as a check. There is one more point that must be noticed before giving the results. Owing to the high specific gravity of sulphuric acid, it seemed hardly safe to use the full quantity of 1000 c. c. of acids stronger than 60 p. c.; and therefore the smaller quantity of 750 c. c. was used; and in order to compare the effect produced by 750 c. c. with that by 1000 c. c., 750 c. c. of 35-p.-c. acid was taken, and its effect observed after correction by the comparison plate. The apparent resistance obtained was 1·66 times the true resistance given by 1000 c. c.; and therefore, of the four solutions containing respectively 70, 80, 90, and 95·5 p. c. anhydrous sulphuric acid (H_2SO_4), only 750 c. c. were taken, and the observed resistance divided by 1·66.

The method of calculating the results is given next. The speed is measured by the number of turns in one second. Each of these numbers, together with the reading of the scale

H_2SO_4 25; H_2O 75. T. 16° C.					
Exp.	*a.*	*b.*	*c.*	*d.*	*e.*
Speed	−16·81	−34·60	+31·59	+14·68	+14·04
Reading	108·7	88·1	165·3	145·8	174·3

in the line below, is the result of at least four observations. *e* is taken with the comparison plate attached. The first four columns are then taken in pairs, and the sum or the difference of the two speeds taken, according as they are of unlike or of like sign, and put in the first column of the next table; then the differences in the readings are taken and put in the second column.

	Speed.	Torsion.	Zero.	$\frac{\text{Speed}}{\text{Torsion}}$.	
a and *b*	17·79	20·6	128·2	·864	
b and *c*	66·19	77·2	128·4	·857	·860
c and *d*	16·91	19·5	128·9	·867	
e	14·04	45·6	128·7	·4795	
		16·3			
		29·3			

$\frac{\cdot 860}{\cdot 4795}=1{\cdot}794$, the corrected resistance of the liquid; 55·80, the corrected conductivity.

The zero in the next column is obtained from the two observations by a simple proportion sum; and the numbers in the last column are obtained as shown, and are a measure of the resistance of the liquid, but uncorrected by the comparison plate. The three figures obtained differ; but the greatest weight is given to the observation at the highest speed, and ·860 is taken as a fair mean. To make use of the observation *e*, the probable zero must be found by examining the numbers already obtained. As they indicate a gradual rise in its position, a number higher than the mean is taken. This shifting in the zero caused more uncertainty than any thing else; and it is this that limits the accuracy of the investigation. The probable zero subtracted from the reading 174·3 gives the torsion 45·6, due to the liquid and to the plate together; but the torsion due to the liquid alone, of the apparent resistance ·860 already found, and with the speed 14·04, is 16·3; and this, subtracted from 45·6, leaves 29·3, the torsion due to the plate alone; and this divided into the speed 14·04 gives the quotient ·4795, an arbitrary number, the changes in which from time to time represent changes in the magnets or wire. From this the corrected resistance 1·794 is found as shown; and its reciprocal is the conductivity. All these numbers are in themselves entirely arbitrary; but they are all comparable with one another; so that any number of liquids may have their conductivities compared with one another, and eventually with that of some standard metal.

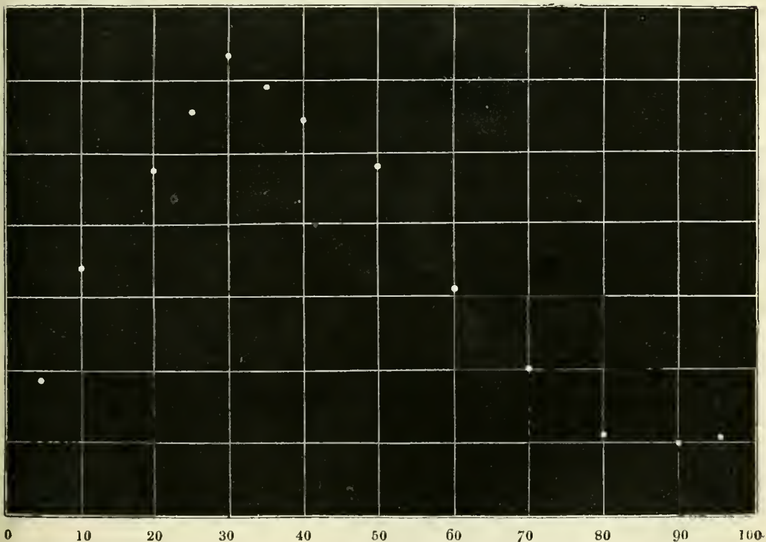
The conductivities of sulphuric acid and of sulphate of copper, the only liquids examined at present, are given in the following Table. The conductivity-curve of sulphuric acid agrees in every important particular with Kohlrausch's, which was obtained with alternating currents, the chief dif-

Per cent.*	Conductivity of H_2SO_4.	T.	Conductivity of $Cu SO_4$.	T.
		°C		°C.
5	18·53 ...	19	1·62 ...	17
10	34·10 ...	17	2·56 ...	17
15	....	...	3·58 ...	17·5
17	....	...	4·47 ...	19
20	47·72 ...	17		
25	55·80 ...	16		
30	63·22 ...	16		
35	58·99 ...	15		
40	54·01 ...	15·5		
50	48·23 ...	16		
60	31·26 ...	16·5		
70	20·34 ...	16		
80	11·61 ...	16·3		
90	10·06 ...	18		
95·5	10·74 ...	18		

ference being rather a sharper rise to and fall from the first maximum; otherwise the position of the two maxima, of the minimum, and of the point of contrary flexure agree most perfectly †. The experiments will be continued on other liquids.

* *Conductivity of Sulphuric Acid and Water.*

Abscissæ are percentages of H_2SO_4. *Ordinates* are conductivities.



† A large quantity of oil of vitriol was boiled in platinum capsules, cooled, and thoroughly mixed. 3·7496 grams gave 8·5114 grams Ba_2SO_4, showing 95·5 per cent. of H_2SO_4. After dilution to obtain the requisite strengths, the clear acids were siphoned off from the deposited sulphate of lead.

XL. *A Question regarding one of the Physical Premises upon which the Finality of Universal Change is based.* *By* S. Tolver Preston*.

I SHOULD like to add a few remarks by way of appendix to my paper on the subject of recurring changes in the universe ('Nature,' March 20, 1879, and Philosophical Magazine, August, 1879). All I wish to deal with is one point regarding the physical premises upon which the view of finality in the universe is based; and the fact of having proposed a theory of the æther † (a body which affects this question in the most intimate manner) may perhaps warrant my saying a few words. It is of course not my intention here to question in any way the conclusion of the tendency to the establishment of a general equilibrium of temperature in the universe. My object is rather to suggest a reconciliation of this with a means for recurring change.

The view of those who believe that physical evidence points to ultimate finality and cessation of life in the universe, could not perhaps well be summarized with greater clearness than in the following passage from Prof. Balfour Stewart's work 'The Conservation of Energy' (3rd edition, 1874) under heading "Probable Fate of the Universe," viz.:—"We have spoken already about a medium pervading space, the office of which appears to be to degrade and ultimately extinguish all differential motion, just as it tends to reduce and ultimately equalize all difference of temperature. Thus the universe would ultimately become an equally heated mass, utterly worthless as far as the production of work is concerned, since such production depends upon difference of temperature. Although, therefore, in a strictly mechanical sense there is a conservation of energy; yet, as regards usefulness or fitness for living beings, the energy of the universe is in process of deterioration. Universally diffused heat forms what we may call the great waste-heap of the universe, and this is growing larger year by year. At present it does not sensibly obtrude itself; but who knows that the time may not arrive when we shall be practically conscious of its growing bigness. It will be seen that in this chapter we have regarded the universe, not as a collection of matter, but rather as an energetic agent—in fact, as a lamp. Now, it has been well pointed out by Thomson that, looked at in this light, the universe is a system that had a beginning and must have an end; for a process

* Communicated by the Author.

† Phil. Mag. Sept. & Nov. 1877, Feb. 1878; 'Nature,' Jan. 15th, 1880, &c.

of degradation cannot be eternal. If we could view the universe as a candle not lit, then it is perhaps conceivable to regard it as having been always in existence; but if we regard it rather as a candle that has been lit, we become absolutely certain that it cannot have been burning from eternity, and that a time will come when it will cease to burn. We are led to look to a beginning in which the particles of matter were in a diffuse chaotic state, but endowed with the power of gravitation; and we are led to look to an end in which the whole universe will be one equally heated inert mass, and from which everything like life or motion or beauty will have utterly gone away." [P. 153.]

Here the key to the whole conclusion regarding finality in universal change would seem to rest in the opening sentence, viz.:—"We have spoken already about a medium pervading space, the office of which *appears to be to degrade and ultimately extinguish all differential motion.*" Does not this, however, assume as a basis the old *statical* theory of a stagnant æther whose parts are normally at rest, and the function of which of course would be to degrade all kinds of motions? How about the case, however (it may be asked), under the *dynamical* theory?—which makes the æther an active substance whose particles are normally in rapid motion, and therefore which would naturally tend continually to stir up and maintain motion in the parts (molecules, &c.) of the universe immersed in the æther, and which motion developed in molecules might perhaps (as a secondary consequence) conduce to a translatory motion of large-scale (stellar) masses, as we actually observe. A dynamical view of the æther cannot, at least, be proved to be untenable; and therefore the possible consequences of such a view (as will no doubt be admitted) could not be fitly excluded from any premises upon which a trustworthy conclusion was intended to be based. A dynamical or kinetic theory of the æther (whose particles radiate automatically in streams with a very great range of path)* affords, as I have pointed out, the only means of getting rid of the numerous and unsatisfactory postulates attaching to Le Sage's theory of gravitation, the ingenious idea involved in which is now accepted by some eminent physicists as a hopeful explanation of the phenomena of gravitation. In fact modern science goes notoriously to reverse all the old *statical* views, and change them into active dynamical theorems.

If once a kinetic theory of the æther be entertained, then a kinetic theory applied to the universe (immersed in the æther)

* Every molecule in the universe (even in the internal parts of masses) is thus acted on by these perturbing streams.

appears inevitable, the stellar or cosmical bodies of the universe being then regarded merely as larger-scale masses immersed in a smaller-scale gas—the universe coming to be comparable (in principle at least) to a gigantic gas *, of which the stellar masses represent the molecules. Drawing an analogy, therefore, from the kinetic theory of gases, this view would accordingly point to the conclusion that, consistently with unlimited variation in the energies of individual stellar masses, the general distribution of energy in the universe may be at present uniform, if sufficiently large samples be taken. For we know that in the general case of portions of matter moving under the kinetic theory (such as in a gas), equilibrium of temperature, although perpetually tending to establish itself, is only possible *per unit of volume*, and that individual molecules may possess all sorts of temperatures. It is only when sufficiently large samples of the gas (small to ordinary standards of size no doubt, but each containing

* For the further development of this idea (which may well appear unfitting at the first view), I must refer to the previous papers above cited, as I wish to avoid repetition here as far as possible. Naturally the first thought presenting itself to the mind, in comparing the universe to a gas, may be an enormous frequency of collisions occurring among the parts. But this evidently need not be, since the number of collisions may be reduced to any extent by sufficiency of vacant space for the cosmical masses to move in, which appears to be relatively almost illimitable in the case of the universe. It might also appear at first sight that the translatory velocity of the stellar masses (as far as we can observe this) is much too great to be accounted for by any theory of equilibrium of motion between them and the æther in which they are immersed, or arising, as a possible secondary consequence, out of the motion developed in the constituent molecules of these masses by the æther. But then it may be replied to this, that, in the first place, we know nothing about either the *mean* velocity or the *mean* mass of the cosmical bodies, which is the essential point involved here; and, secondly, we can only perceive *luminous* masses (which may be in the minority). Also our field of view is notoriously very circumscribed. No doubt a theory (or suggestion) like the above will not commend itself at an initial glance. It may even seem extravagant at first. But then most *truths* appear strange at first sight, or this, as a fact of notoriety, may be worth keeping in remembrance.

The comparison of the universe to a lighted "candle" (employed in the passage quoted) seems in point of principle scarcely legitimate or exact, in so far as a lighted candle necessarily implies waste from chemical action, which of course essentially gives an idea of finality. A comparison to a hot body would not imply waste or change of this kind; and if we conceive to ourselves a hot body placed under such conditions that it cannot cool, we should then have no change. My contention is that the universe is to be regarded as such a hot body which (as a whole) cannot cool, but whose parts have differential temperatures, owing to differential motion under the kinetic theory. It is, in one sense at least, much as if we were inside a nebula of a "nébulosité, tellement diffuse que l'on en pourrait à peine soupçonner l'existence," to quote an expression of Laplace.

many millions of molecules) are taken for comparison, that the temperatures of the samples can be said to be equal, or that equilibrium of temperature can be said to exist. My contention (or suggestion) therefore is—magnify the scale, and apply in broad principle the same general considerations to the universe. In other words, apply the great dynamical theorem developed by Joule, Clausius, and Maxwell on a grand scale. It is at least so far true, that dynamical considerations or mechanical principles are generally regarded as independent of *scale*, or that this could not be a point affecting their validity, whatever other reasonable grounds of objection or difficulties might arise. For of course in a suggestion having any points of novelty about it, it is scarcely to be expected that all will appear smooth and straightforward at the outset. A little unbiased thought may even be necessary to make it appear worthy of examination. It is at least always conceivable beforehand, that in so wide a subject some physical point affecting the resulting conclusion of finality in the universe may have escaped notice. And the definite manner (almost approaching to an air of certainty) with which this ultimate destiny of the universe is sometimes enunciated, would seem to warrant great care in weighing every possible eventuality in the premises upon which this view is grounded. Sir W. Grove remarks on this point :—" The notion that the universe is gradually equalizing its heat and tending to a chaos of uniform temperature and equilibrium of force, although it may be supported by the phenomena we see immediately around us, seems to me like those views of the instability of the solar system which Laplace negatived, and would require far more cogent proof than any at present given" ('Correlation of Physical Forces,' 6th Edition, page 69).

It may, however, be remarked that if the idea suggested here (and in former essays) should ultimately turn out to have a germ of truth, it would serve to some extent the purpose of reconciling rival views. For while recurring change would still be possible (or, in Sir W. Grove's words, " phenomena seeming to tend in one direction will turn out to be recurrent, though never absolutely identical in their recurrence," page 69), nevertheless equilibrium of temperature would actually exist *in a certain sense*, (*i. e.* per unit of volume) * in the universe ; but this would not be inconsistent with the continuance of conditions necessary for life.

London, Oct. 1880.

* If the universe be regarded as practically unlimited in extent, then the size of such a unit of volume, though great compared with a planetary system, would become (relatively speaking) indefinitely small. Hence the theorem that equilibrium (or uniformity) of temperature tends to be

XLI. *On Steady Motion in an Incompressible Viscous Fluid.*

By THOMAS CRAIG, *Ph.D., U.S. Coast and Geodetic Survey, formerly Fellow of the Johns Hopkins University, Baltimore, Md., U. S. A.**

IN the following paper an attempt has been made to give the theory of steady motion in an ordinary viscous fluid, and the results of the general investigation applied to the case when there is a solid body of given form immersed in the fluid, and the fluid moves subject to the condition that at infinity all the flow is parallel to the axis of z.

The general equations of motion of an incompressible viscous fluid are well known to be

$$\left.\begin{aligned}\rho\frac{\mathrm{D}u}{dt}&=\rho\mathrm{X}-\frac{dp}{dx}+\mu\nabla^2u,\\ \rho\frac{\mathrm{D}v}{dt}&=\rho\mathrm{Y}-\frac{dp}{dy}+\mu\nabla^2v,\\ \rho\frac{\mathrm{D}w}{dt}&=\rho\mathrm{Z}-\frac{dp}{dz}+\mu\nabla^2w,\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad (1)$$

maintained, and actually *is* maintained in the universe, would become strictly true, in so far as the limits within which such uniformity of temperature is non-existent would be (relatively speaking) indefinitely small.

The late Prof. Clerk Maxwell remarks ('Theory of Heat,' p. 163):—"The transference of heat, therefore, from one body of the system to another always increases the 'entropy' of the system. Clausius expresses this by saying that the entropy of the system always tends towards a maximum value." It appears, therefore, that, on the above view that the universe is already in a state of equilibrium of temperature, the "entropy" of the universe would already have reached a maximum value, which continually tends to be maintained. The theory of finality in the universe seems to have been discussed with considerable interest in Germany. A critical notice of this subject may be found in Lange's *Geschichte des Materialismus* (of which I believe an English translation now exists).

It should be mentioned [as also noticed in my previous essays] that Dr. Croll, in the Phil. Mag. for May 1868 and July 1878, also Mr. Johnstone Stoney (Proceedings of the Royal Society 1868), have published papers dealing with the eventuality of encounters among the stars, and suggested views which may, as far as they go, be regarded as consistent with the development of an encircling theory suggested in this and my former essays. Also the fact of the present theory having been arrived at before seeing the papers above cited, naturally tended rather to afford some confidence in the result.

The entertaining of the above theory would be no more than admitting the possible application of the principle of evolution to universal changes as well as to planetary changes: the birth and death of worlds comparable to the birth and death of individuals. Or the secular fluctuations of life in the cosmical units of the universe would be paralleled by those of the individuals of a planet—while the conservation of life has its correlative in the conservation of energy and of matter.

* Communicated by A. G. Greenhill, Esq.

in which u, v, w denote the component velocities of a fluid particle in the directions of x, y, z respectively, ρ is the density of the fluid, μ the coefficient of viscosity, and

$$\frac{\mathrm{D}}{dt} \equiv \frac{d}{dt} + u\frac{d}{dx} + v\frac{d}{dy} + w\frac{d}{dz},$$

$$\nabla^2 = \frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}.$$

Denoting the components of the spin by ξ, η, ζ, we have

$$\left.\begin{aligned} \xi &= \tfrac{1}{2}\left(\frac{dw}{dy} - \frac{dv}{dz}\right), \\ \eta &= \tfrac{1}{2}\left(\frac{du}{dz} - \frac{dw}{dx}\right), \\ \zeta &= \tfrac{1}{2}\left(\frac{dv}{dx} - \frac{du}{dy}\right). \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

We may observe just here, that from equations (2) we can obtain

$$\left.\begin{aligned} \nabla^2 u &= \frac{d\eta}{dz} - \frac{d\zeta}{dy}, \\ \nabla^2 v &= \frac{d\zeta}{dx} - \frac{d\xi}{dz}, \\ \nabla^2 w &= \frac{d\xi}{dy} - \frac{d\eta}{dx}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In the case of steady motion we have

$$\frac{du}{dt} = \frac{dv}{dt} = \frac{dw}{dt} = 0;$$

and equations (1) thus assume the form

$$\left.\begin{aligned} \rho\left\{u\frac{du}{dx} + v\frac{du}{dy} + w\frac{du}{dz} - \mathrm{X}\right\} + \frac{dp}{dx} &= \mu\nabla^2 u, \\ \rho\left\{u\frac{dv}{dx} + v\frac{dv}{dy} + w\frac{dv}{dz} - \mathrm{Y}\right\} + \frac{dp}{dy} &= \mu\nabla^2 v, \\ \rho\left\{u\frac{dw}{dx} + v\frac{dw}{dy} + w\frac{dw}{dz} - \mathrm{Z}\right\} + \frac{dp}{dz} &= \mu\nabla^2 w. \end{aligned}\right\} \quad . \quad (4)$$

Assume that the forces X, Y, Z possess a potential V, and write

$$q^2 = u^2 + v^2 + w^2:$$

these equations can now be transformed into the following:—

$$\left.\begin{aligned} \frac{dP}{dx} &= 2(v\zeta - w\eta) + \tau\nabla^2 u, \\ \frac{dP}{dy} &= 2(w\xi - u\zeta) + \tau\nabla^2 v, \\ \frac{dP}{dz} &= 2(u\eta - v\xi) + \tau\nabla^2 w, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where $\tau = \frac{\mu}{\rho}$ is the "kinematic coefficient of viscosity," and

$$P = \int \frac{dp}{\rho} + V + \tfrac{1}{2}q^2.$$

For $\mu = 0$, these become the equations given by Lamb in the 'Proceedings' of the London Mathematical Society. In the case of $\mu = 0$, we know (*vide* Lamb's 'Treatise on Fluid Motion,' page 173)

"that
$$u\frac{dP}{dx} + v\frac{dP}{dy} + w\frac{dP}{dz} = 0,$$

$$\xi\frac{dP}{dx} + \eta\frac{dP}{dy} + \zeta\frac{dP}{dz} = 0;$$

"so that the surfaces

$$P = \text{constant}$$

"contain both stream- and vortex-lines. Further, denoting by dn an element of the normal to such a surface at any point, we have

$$\frac{dP}{dn} = q\omega \sin\theta\,;$$

where ω is the spin, and θ is the angle between the stream-line and vortex-line at the point where the normal is drawn.

"The conditions, then, that a given state of motion of a perfect fluid may be a possible state of steady motion are as follows:—It must be possible to draw in the fluid an infinite number of surfaces each of which is covered by a network of stream-lines and vortex-lines; and the product $q\omega \sin\theta\, dn$ must be constant over each such surface, dn being the length of the normal drawn to a consecutive surface of the system."

For the case of a viscous fluid, the reductions are as simple as those which lead to the above results. To the equations of motion (5) it is of course necessary to add the equation of continuity,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0;$$

we have also the analogous equation for ξ, η, ζ,

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}=0.$$

From equations (5) we obtain

$$\left.\begin{aligned}
&u\left\{\frac{d\mathrm{P}}{dx}-2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\right\}+v\left\{\frac{d\mathrm{P}}{dy}-2\tau\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)\right\}\\
&\qquad\qquad+w\left\{\frac{d\mathrm{P}}{dz}-2\tau\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)\right\}=0,\\
&\xi\left\{\frac{d\mathrm{P}}{dx}-2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\right\}+\eta\left\{\frac{d\mathrm{P}}{dy}-2\tau\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)\right\}\\
&\qquad\qquad+\zeta\left\{\frac{d\mathrm{P}}{dz}-2\tau\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)\right\}=0.
\end{aligned}\right\} \quad . \quad (6)$$

If we have a surface $\Theta=$ const. defined by the differential equations

$$\left.\begin{aligned}
\frac{d\Theta}{dx}&=\frac{d\mathrm{P}}{dx}-2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right),\\
\frac{d\Theta}{dy}&=\frac{d\mathrm{P}}{dy}-2\tau\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right),\\
\frac{d\Theta}{dz}&=\frac{d\mathrm{P}}{dz}-2\tau\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right),
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (7)$$

these equations may be written

$$\left.\begin{aligned}
u\frac{d\Theta}{dx}+v\frac{d\Theta}{dy}+w\frac{d\Theta}{dz}&=0,\\
\xi\frac{d\Theta}{dx}+\eta\frac{d\Theta}{dy}+\zeta\frac{d\Theta}{dz}&=0,
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (8)$$

and we have, as a result, that there exist in the fluid an infinite number of surfaces $\Theta=$constant, each of which is covered by a network of stream-lines and vortex-lines.

The expression

$$\left\{\frac{d\mathrm{P}}{dx}-2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\right\}dx+\text{\&c.}$$

is not an exact differential unless $\nabla^2\xi=0$, $\nabla^2\eta=0$, $\nabla^2\zeta=0$; and consequently equations (7) do not always hold. Equations (6), however, must always hold, as they are obtained independently of the supposition contained in (7). Equations (6) may be written, for brevity, in the form

$$\left.\begin{aligned}
u\Phi_1+v\Phi_2+w\Phi_3&=0,\\
\xi\Phi_1+\eta\Phi_2+\zeta\Phi_3&=0.
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Write

$$\Omega=\sqrt{\Phi_1^2+\Phi_2^2+\Phi_3^2}\,;$$

then we see from (9) that the stream-lines and vortex-lines always lie on a surface the direction-cosines of a normal to which are

$$\frac{\Phi_1}{\Omega},\quad \frac{\Phi_2}{\Omega},\quad \frac{\Phi_3}{\Omega}.$$

We shall confine ourselves at present to the case where

$$\Phi_1=\frac{d\Theta}{dx},\ \&c.,$$

i. e.

$$\Phi_1 dx+\Phi_2 dy+\Phi_3 dz$$

is an exact differential $d\Theta$. It is clear that, in order that this quantity may be an exact differential, it is necessary only that

$$2\tau\left\{\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)dx+\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)dy+\left(\frac{d\xi}{dy}-\frac{d\zeta}{dx}\right)dz\right\}$$

or, briefly,

$$\mathrm{A}dx+\mathrm{B}dy+\mathrm{C}dz$$

shall be such a quantity. The conditions which have to be fulfilled on this supposition are well known to be

$$\frac{d\mathrm{A}}{dy}-\frac{d\mathrm{B}}{dx}=0,\ \&c.,$$

or, substituting for A, B, C their values,

$$\nabla^2\xi=0,\quad \nabla^2\eta=0,\quad \nabla\zeta=0. \quad . \quad . \quad . \quad (10)$$

Since, now,

$$\left.\begin{aligned}\frac{d\Theta}{dx}&=2(v\zeta-w\eta),\\ \frac{d\Theta}{dy}&=2(w\xi-u\zeta),\\ \frac{d\Theta}{dz}&=2(u\eta-v\xi),\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

equations (5) become

$$\left.\begin{aligned}\frac{d(\mathrm{P}-\Theta)}{dx}&=2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right),\\ \frac{d(\mathrm{P}-\Theta)}{dy}&=2\tau\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right),\\ \frac{d(\mathrm{P}-\Theta)}{dz}&=2\tau\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right).\end{aligned}\right\} \quad . \quad . \quad . \quad (12)$$

Differentiating these for x, y, and z respectively and adding, we have

$$\nabla^2(\mathrm{P}-\Theta)=0, \qquad (13)$$

or

$$\nabla^2\mathrm{P}=\nabla^2\Theta. \qquad (14)$$

Denoting by θ the angle between the stream-line and the vortex-line at the point where the normal n is drawn to the surface $\Theta=$ const., we have, since

$$\omega=\sqrt{\xi^2+\eta^2+\zeta^2}.$$

and

$$q\omega \sin\theta=\sqrt{(v\zeta-w\eta)^2+(w\xi-u\zeta)^2+(u\eta-v\xi)^2},$$

$$\frac{d\Theta}{dn}=q\omega\sin\theta. \qquad (15)$$

From this, as in the case of no viscosity, follows that the product

$$q\omega\sin\theta\, dn$$

must be constant over each of the surfaces $\Theta=$ const., dn denoting the normal drawn to the consecutive surface.

For the determination of the pressure p it will be convenient to resume equations (4). Since $u=q\dfrac{dx}{ds}$, &c., these become

$$\left.\begin{aligned}\frac{1}{\rho}\frac{dp}{dx}&=-\frac{d\mathrm{V}}{dx}-q\frac{du}{ds}+\tau\nabla^2u,\\ \frac{1}{\rho}\frac{dp}{dy}&=-\frac{d\mathrm{V}}{dy}-q\frac{dv}{ds}+\tau\nabla^2v,\\ \frac{1}{\rho}\frac{dp}{dz}&=-\frac{d\mathrm{V}}{dz}-q\frac{dw}{ds}+\tau\nabla^2w.\end{aligned}\right\} \qquad (16)$$

Multiplying these by $\dfrac{dx}{ds}, \dfrac{dy}{ds}, \dfrac{dz}{ds}$ respectively and adding these results,

$$\frac{1}{\rho}\frac{dp}{ds}=\frac{d(\mathrm{P}-\Theta)}{ds}-\frac{d\mathrm{V}}{ds}-\tfrac{1}{2}\frac{d(q^2)}{ds};$$

from which, since ρ is constant,

$$p=\rho\left\{(\mathrm{P}-\Theta)-\mathrm{V}-\tfrac{1}{2}q^2\right\}+\mathrm{C}. \qquad (17)$$

As we have integrated along the stream-line $\int ds$, the quantity C is only constant for this particular line.

Let us assume that a solid sphere is immersed in the fluid, the latter moving past the sphere in such a manner that the

motion is steady. If we write

$$\left.\begin{aligned} u&=\frac{d\phi}{dx}+\frac{dW}{dy}-\frac{dV}{dz}, \\ v&=\frac{d\phi}{dy}+\frac{dU}{dz}-\frac{dW}{dx}, \\ w&=\frac{d\phi}{dz}+\frac{dV}{dx}-\frac{dU}{dy}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (18)$$

the equation of continuity

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0$$

will be satisfied, provided ϕ satisfies the condition

$$\nabla^2\phi=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

The remaining functions U, V, W must, as is well known, satisfy the following conditions:—

$$\left.\begin{aligned} &\nabla^2 U=-2\xi, \quad \nabla^2 V=-2\eta, \quad \nabla^2 W=-2\zeta, \\ &\frac{dU}{dx}+\frac{dV}{dy}+\frac{dW}{dz}=0. \end{aligned}\right\} \quad . \quad (20)$$

The origin of coordinates is to be taken at the centre of the sphere; then, if r denote the distance from the centre to any point $x, y, z,$ we have for the transformation to polar coordinates,

$$\begin{aligned} x&=r\sin\chi\cos\psi, \\ y&=r\sin\chi\sin\psi, \\ z&=r\cos\chi; \end{aligned}$$

the equation $\nabla^2\phi=0$ now becomes

$$r\frac{d^2(r\phi)}{dr^2}+\frac{1}{\sin\chi}\frac{d}{d\chi}\left(\sin\chi\frac{d\phi}{d\chi}\right)+\frac{1}{\sin^2\chi}\frac{d^2\phi}{d\psi^2}=0.$$

The most general solution of this in spherical harmonics is

$$\begin{aligned} \phi=\alpha_0 Y_0+\alpha_1 rY_1+\alpha_2 r^2 Y_2+\dots \\ +\beta_0\frac{Y_0}{r}+\beta_1\frac{Y_1}{r^2}+\beta_2\frac{Y_2}{r^3}+\dots, \end{aligned}$$

or

$$\phi=\sum_0^n\left(\alpha_i r^i+\frac{\beta_i}{r^{i+1}}\right)Y_i,$$

Y_i being a surface spherical harmonic of degree i. Write $\phi_i=\alpha_i r^i Y_i$; ϕ_i is a solid spherical harmonic of degree i; this gives

$$\phi=\sum_0^n\left(1+\frac{\beta_i}{\alpha^i}r^{-(2i+1)}\right)\phi_i. \quad . \quad . \quad . \quad . \quad (21)$$

For the determination of the constants α and β we have only one condition; so that at most we can only find one of them in terms of the other. This condition is obtained by equating to zero the normal velocity at the surface of the sphere; if the radius of the sphere is a, we must have

$$\frac{d\phi}{dr}=0 \text{ for } r=a.$$

Differentiating ϕ with respect to r gives

$$\frac{d\psi}{dr}=\sum_0^n(i\alpha_i r^{i-1}-(i+1)\beta_i r^{-(i+2)})Y_i;$$

equating this to zero and making $r=a$, we find

$$i\alpha_i a^{i-1}=(i+1)\beta_i a^{-(i+2)},$$

from which

$$\beta_i=\frac{i}{i+1}\alpha_i a^{2i+1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Substituting this value of β_i in the last form of ϕ, we have

$$\phi=\sum_0^n\left\{1+\frac{i}{i+1}\left(\frac{a}{r}\right)^{2i+1}\right\}\phi_i. \quad . \quad . \quad (23)$$

For the determination of the functions U, V, W, we have

$$\nabla^2 U=-2\xi, \quad \nabla^2 V=-2\eta, \quad \nabla^2 W=-2\zeta,$$

with the condition

$$\frac{dU}{dx}+\frac{dV}{dy}+\frac{dW}{dz}=0.$$

Equations (10) hold for the particular case of motion that we are studying; and therefore

$$\nabla^2(\nabla^2 U)=0, \quad \nabla^2(\nabla^2 V)=0, \quad \nabla^2(\nabla^2 W)=0.$$

The functions U, V, W can be expressed in a series the terms of which depend upon the general spherical harmonics. The following solution is merely a generalization of one given by Mr. J. G. Butcher in the 'Proceedings' of the London Mathematical Society. Mr. Butcher says, in his article "On Viscous Fluids in Motion," that the method is due to Stokes, to whose article, however, I am not able to refer.

The equations to be solved are of the form

$$\left.\begin{aligned}\nabla^2 S&=-s,\\ \nabla^2 s&=0.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

In the problem solved by Mr. Butcher the solution is made to

depend upon zonal harmonics; merely generalizing his solution, I am led to write for S the series

$$S = \frac{a_0 S_0}{r} + \frac{a_1 S_1}{r^3} + \frac{a_2 S_2}{r^5} + \ldots$$
$$+ \frac{b_1 S_1}{r} + \frac{b_2 S_2}{r^3} + \frac{b_3 S_3}{r^5} + \ldots,$$

or simply

$$S = \sum_0^\infty \frac{a_i + b_i r^2}{r^{2i+1}} S_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

S_i being a solid spherical harmonic of degree i. For brevity, make

$$R_i = \frac{a_i + b_i r^2}{r^{2i+1}};$$

then

$$S = \sum_0^\infty R_i S_i. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

In order to find the values of the constants, it is necessary to take into account the conditions to be fulfilled at the surface of the sphere, *i. e.* at the finite bounding surface of the fluid. These are

$$\left.\begin{array}{l} u=0, \\ v=0, \\ w=0, \end{array}\right\} \text{ for } r=a. \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

We must first determine the function U, V, W, however, before these boundary conditions can be introduced. The following method is due to Borchardt, and is given in the *Monatsber. der Berlin. Akad.* for 1873. If we have a function s of x, y, z satisfying the equation

$$\nabla s^2 = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

and four other functions connected with s by the relations

$$\left.\begin{array}{l} s_0 = s + x\dfrac{ds}{dx} + y\dfrac{ds}{dy} + z\dfrac{ds}{dz}, \\ s_1 = z\dfrac{ds}{dy} - y\dfrac{ds}{dz}, \\ s_2 = x\dfrac{ds}{dz} - z\dfrac{ds}{dx}, \\ s_3 = y\dfrac{ds}{dx} - x\dfrac{ds}{dy}, \end{array}\right\} \quad . \quad . \quad . \quad (29)$$

then there results

$$\left.\begin{aligned}&\nabla^2 s_0=\nabla^2 s_1=\nabla^2 s_2=\nabla^2 s_3=0,\\ &\frac{ds_1}{dx}+\frac{ds_2}{dx}+\frac{ds_3}{dx}=0,\end{aligned}\right\} \quad . \quad . \quad . \quad (30)$$

and also

$$\left.\begin{aligned}&\frac{ds_3}{dy}-\frac{ds_2}{dz}=\frac{ds_0}{dx},\\ &\frac{ds_1}{dz}-\frac{ds_3}{dx}=\frac{ds_0}{dy},\\ &\frac{ds_2}{dx}-\frac{ds_1}{dy}=\frac{ds_0}{dz}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (31)$$

If $s=\phi_i$, then

$$s_0=(i+1)\phi_i \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

Since the functions s_1, s_2, s_3 fulfil all the conditions to which ξ, η, ζ are subject, we may write

$$\xi=s_1, \quad \eta=s_2, \quad \zeta=s_3, \quad . \quad . \quad . \quad . \quad (33)$$

s itself being still an arbitrary function. We can consequently write

$$\left.\begin{aligned}&U=z\frac{dS}{dy}-y\frac{dS}{dz}, \quad \xi=z\frac{ds}{dy}-y\frac{ds}{dz},\\ &V=x\frac{dS}{dz}-z\frac{dS}{dx}, \quad \eta=x\frac{ds}{dz}-z\frac{ds}{dx},\\ &W=y\frac{dS}{dx}-x\frac{dS}{dy}, \quad \zeta=y\frac{ds}{dx}-x\frac{ds}{dy},\end{aligned}\right\} \quad . \quad . \quad (34)$$

when all the conditions to which these quantities are subject will be fulfilled, provided

$$\nabla^2 S=-s \text{ and } \nabla^2 s=0.$$

Using the solution already given for the determination of S, we can now write

$$U=\sum_0^\infty R_i\left(z\frac{dS_i}{dy}-y\frac{dS_i}{dz}\right) \text{ \&c.}, \quad . \quad . \quad . \quad (35)$$

since

$$z\frac{dR_i}{dy}-y\frac{dR_i}{dz}=0 \text{ \&c.}$$

Instead of (35), we may write

$$\left.\begin{aligned} U_i &= z\frac{dS^i}{dy} - y\frac{dS_i}{dz}, \\ V_i &= x\frac{dS_i}{dz} - z\frac{dS_i}{dx}, \\ W_i &= y\frac{dS_i}{dx} - x\frac{dS_i}{dy}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (36)$$

and

$$U = \sum_0^\infty R_i U_i, \quad V = \sum_0^\infty R_i V_i, \quad W = \sum_0^\infty R_i W_i. \quad . \quad (37)$$

We have now for u, v, w

$$\left.\begin{aligned} u &= \frac{d\phi}{dx} + \frac{d}{dy}\Sigma R_i W_i - \frac{d}{dz}\Sigma R_i V_i, \\ v &= \frac{d\phi}{dy} + \frac{d}{dz}\Sigma R_i U_i - \frac{d}{dx}\Sigma R_i W_i, \\ w &= \frac{d\phi}{dz} + \frac{d}{dx}\Sigma R_i V_i - \frac{d}{dy}\Sigma R_i U_i. \end{aligned}\right\} \quad . \quad . \quad (38)$$

Since ϕ is expressed by a series of the same form as those giving U, V, and W, we may write

$$\phi = \sum_0^\infty L_i \phi_i.$$

For the surface conditions we now have i groups of equations, of which the following is the type:—

$$\left.\begin{aligned} \frac{d(L_i\phi_i)}{dx} + \frac{d(R_i W_i)}{dy} - \frac{d(R_i V_i)}{dz} &= 0; \\ \frac{d(L_i\phi_i)}{dy} + \frac{d(R_i U_i)}{dz} - \frac{d(R_i W_i)}{dx} &= 0; \\ \frac{d(L_i\phi_i)}{dz} + \frac{d(R_i V_i)}{dx} - \frac{d(R_i U_i)}{dy} &= 0. \end{aligned}\right\} \quad . \quad . \quad (39)$$

The first one of these can be written in the form

$$\left.\begin{aligned} & L^i\frac{d\phi_i}{dx} + R_i\left(\frac{dW_i}{dy} - \frac{dV}{dz}\right) \\ & = V_i\frac{dR_i}{dz} - W_i\frac{dR_i}{dy} - \phi_i\frac{dL_i}{dx}. \end{aligned}\right\} \quad . \quad . \quad . \quad (40)$$

Now

$$\frac{dW_i}{dy} = \frac{dS_i}{dx} + y\frac{d^2S_i}{dx\,dy} - x\frac{d^2S_i}{dyz};$$

$$\frac{dV_i}{dz} = x\frac{d^2S_i}{dz^2} - z\frac{d^2S_i}{dx\,dz} - \frac{dS_i}{dx};$$

therefore

$$\frac{dW_i}{dy}-\frac{dV_i}{dz}=-x\nabla^2 S_i+\left(x\frac{d}{dx}+y\frac{d}{dy}+z\frac{d}{dz}+2\right)\frac{dS_i}{dx}.$$

But S_i is a solid spherical harmonic of degree i, and therefore satisfies the equations

$$\nabla^2 S_i=0,\quad \left(x\frac{d}{dx}+y\frac{d}{dy}+z\frac{d}{dz}\right)\frac{dS_i}{dx}=(i-1)\frac{dS_i}{dx};$$

and consequently

$$\frac{dW_i}{dy}-\frac{dV_i}{dz}=(i+1)\frac{dS_i}{dx}.$$

Again

$$\begin{aligned} V_i\frac{dR_i}{dz}-W_i\frac{dR_i}{dy}&=\frac{1}{r}\frac{dR_i}{dr}(zV_i-yW_i)\\ &=\frac{1}{r}\frac{dR_i}{dr}\left\{x\left(x\frac{d}{dx}+y\frac{d}{dy}+z\frac{d}{dz}\right)S_i-r^2\frac{dS_i}{dx}\right\}\\ &=\frac{1}{r}\frac{dR_i}{dr}\left\{ixS_i-r^2\frac{dS_i}{dx}\right\}. \end{aligned}$$

Equation (40) now becomes

$$\begin{aligned} L_i\frac{d\phi_i}{dx}&+R_i(i+1)\frac{dS_i}{ds}\\ &=\frac{1}{r}\frac{dR_i}{dr}\left\{ixS_i-r^2\frac{dS_i}{dx}\right\}-\phi_i\frac{dL_i}{dx}. \quad . \quad (41) \end{aligned}$$

The second and third of equations (39) give two others similar to (41). Since

$$\frac{dL_i}{dx}=\frac{dL_i}{dr}\cdot\frac{x}{r},\quad \frac{dL_i}{dy}=\frac{dL_i}{dr}\cdot\frac{y}{r},\quad \frac{dL_i}{dz}=\frac{dL_i}{dr}\cdot\frac{z}{r},$$

these may all be written as

$$\left.\begin{aligned} L_i\frac{d\phi_i}{dx}+\frac{x}{r}\cdot\phi_i\frac{dL_i}{dr}&=i\frac{x}{r}\frac{dR_i}{dr}S_i-\left\{(i+1)S_i+r\frac{dR_i}{dr}\right\}\frac{dS_i}{dx},\\ L_i\frac{d\phi_i}{dy}+\frac{y}{r}\cdot\phi_i\frac{dL_i}{dr}&=i\frac{y}{r}\frac{dR_i}{dr}S_i-\left\{(i+1)S_i+r\frac{dR_i}{dr}\right\}\frac{dS_i}{dy},\\ L_i\frac{d\phi_i}{dz}+\frac{z}{r}\cdot\phi_i\frac{dL_i}{dr}&=i\frac{z}{r}\frac{dR_i}{dr}S_i-\left\{(i+1)S_i+r\frac{dR_i}{dr}\right\}\frac{dS_i}{dz}. \end{aligned}\right\} \quad . \quad (42)$$

These equations are to be satisfied only when $r=a$, and mani-

festly are satisfied only when

$$\left.\begin{aligned}&\phi_i=S_i,\\&L_i=-\left\{(i+1)R_i+r\frac{dR_i}{dr}\right\},\\&\frac{dL_i}{dr}=i\frac{dR_i}{dr},\end{aligned}\right\}\quad . \quad . \quad . \quad (43)$$

which are then the conditions to be fulfilled for $u=v=w=0$. Referring now to the values of R_i and L_i, we have

$$\left.\begin{aligned}&L_i=\frac{2i+1}{i+1},\\&N_i=\frac{a_i+b_ia^2}{a^{2i+1}},\end{aligned}\right\}\text{ for } r=a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

$$\left.\begin{aligned}&r\frac{dR_i}{dr}=\frac{-(2i+1)a_i+(2i-1)b_ia^2}{a^{2i+1}},\\&\frac{dL_i}{dr}=-\frac{i(2i+1)}{i+1}\cdot\frac{1}{a},\end{aligned}\right\}\text{ for } r=a. \quad . \quad (45)$$

Constructing, now, the last of the condition-equations (43), there result

$$\left.\begin{aligned}(2i+1)a_i+(2i-1)b_ia^2&=\frac{2i+1}{i+1}a^{2i+1},\\ia_i+(i-2)b_ia^2&=\frac{2i+1}{i+1}a^{2i+1};\end{aligned}\right\}\quad . \quad (46)$$

from which we derive

$$\left.\begin{aligned}a_i&=\quad\frac{1}{2}\cdot\frac{2i+1}{i+1}a^{2i+1},\\b_i&=-\frac{1}{2}\cdot\frac{2i+1}{i+1}a^{2i-1};\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad (47)$$

and consequently

$$S=\tfrac{1}{2}\sum_0^\infty\frac{2i+1}{i+1}\cdot\left(\frac{a}{r}\right)^{2i-1}\left[\left(\frac{a}{r}\right)^2-1\right]\phi_i \quad . \quad . \quad . \quad (48)$$

From the relation $\nabla^2 S=-s$ we obtain at once

$$s=\tfrac{1}{2}\sum_0^\infty\frac{4i^2-1}{i+1}\cdot\frac{a^{2i-1}}{r^{2i+1}}\cdot\phi_i. \quad . \quad . \quad . \quad . \quad (49)$$

Since S and s are thus fully determined, the velocities u, v, w, and ξ, η, ζ are also known, and the problem of the motion of the fluid particles is solved. We had before

$$u=\sum_0^\infty\left\{L_i\frac{d\phi_i}{dx}+\phi_i\frac{dL_i}{dx}+R_i\left(\frac{dW_i}{dy}-\frac{dV_i}{dz}\right)+W_i\frac{dR_i}{dy}-V_i\frac{dR_i}{dz}\right\},$$

or

$$u=\sum_0^\infty\left\{\mathrm{L}_i\frac{d\phi_i}{dx}+\frac{x}{r}\phi_i\frac{d\mathrm{L}_i}{dr}+\left[(i+1)\mathrm{R}_i+r\frac{d\mathrm{R}_i}{dr}\right]\frac{d\mathrm{S}_i}{dx}-i\frac{x}{r}\mathrm{S}_i\frac{d\mathrm{R}}{dr}\right\},$$

with two similar expressions for v and w; these become now

$$\left.\begin{aligned}
u&=\sum_0^\infty\left\{\frac{d\phi_i}{dx}\left[\mathrm{L}_i+(i+1)\mathrm{R}_i+r\frac{d\mathrm{R}_i}{dr}\right]+\frac{x}{r}\phi_i\frac{d}{dr}(\mathrm{L}_i-i\mathrm{R}_i)\right\},\\
v&=\sum_0^\infty\left\{\frac{d\phi_i}{dy}\left[\mathrm{L}_i+(i+1)\mathrm{R}_i+r\frac{d\mathrm{R}_i}{dr}\right]+\frac{y}{r}\phi_i\frac{d}{dr}(\mathrm{L}_i-i\mathrm{R}_i)\right\},\\
w&=\sum_0^\infty\left\{\frac{d\phi_i}{dz}\left[\mathrm{L}_i+(i+1)\mathrm{R}_i+r\frac{d\mathrm{R}_i}{dr}\right]+\frac{z}{r}\phi_i\frac{d}{dr}(\mathrm{L}_i-i\mathrm{R}_i)\right\};
\end{aligned}\right\}\cdot(50)$$

or, since

$$\mathrm{L}_i=1+\frac{i}{i+1}\left(\frac{a}{r}\right)^{2i+1},$$

$$\mathrm{R}_i=\frac{1}{2}\cdot\frac{2i+1}{i+1}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right),$$

these can be written

$$\left.\begin{aligned}
u=\sum_0^\infty\Bigg\{&\frac{d\phi_i}{dx}\left[1-\frac{i(2i-1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\right]\\
&+x\phi_i\frac{i(2i-1)(2i+1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\Bigg\},\\
v=\sum_0^\infty\Bigg\{&\frac{d\phi_i}{dy}\left[1-\frac{i(2i-1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\right]\\
&+y\phi_i\frac{i(2i-1)(2i+1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\Bigg\},\\
w=\sum_0^\infty\Bigg\{&\frac{d\phi_i}{dz}\left[1-\frac{i(2i-1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\right]\\
&+z\phi_i\frac{i(2i-1)(2i+1)}{2(i+1)}\left(\frac{a}{r}\right)^{2i-1}\left(\left(\frac{a}{r}\right)^2-1\right)\Bigg\}.
\end{aligned}\right\}(51$$

The equations giving ξ, η, ζ are those in the second group of (34); from these, with reference to (49), we obtain at once

$$\left.\begin{aligned}
\xi&=\tfrac{1}{2}\sum_0^\infty\frac{(2i+1)(2i-1)}{i+1}\cdot\frac{a^{2i-1}}{r^{2i+1}}\left(z\frac{d\phi_i}{dy}-y\frac{d\phi_i}{dz}\right),\\
\eta&=\tfrac{1}{2}\sum_0^\infty\frac{(2i+1)(2i-1)}{i+1}\cdot\frac{a^{2i-1}}{r^{2i+1}}\left(x\frac{d\phi_i}{dz}-z\frac{d\phi_i}{dx}\right),\\
\zeta&=\tfrac{1}{2}\sum_0^\infty\frac{(2i+1)(2i-1)}{i+1}\cdot\frac{a^{2i-1}}{r^{2i+1}}\left(y\frac{d\phi_i}{dx}-x\frac{d\phi_i}{dy}\right).
\end{aligned}\right\}\cdot\ (52)$$

From equations (34) we have

$$x\xi+y\eta+z\zeta=0,$$

$$\xi\frac{ds}{dx}+\eta\frac{ds}{dy}+\zeta\frac{ds}{dz}=0;$$

the vortex-lines therefore lie on the surface $s=$const., and also on the sphere $r=$ const. For the particular case when the flow at an infinitely great distance from the sphere is parallel to the axis of x, all of the functions vanish except ϕ_1; then writing

$$\phi_1=-\lambda x,$$

we have

$$\left.\begin{aligned} u&=\lambda\left\{1-\frac{3}{4}\frac{a}{r}-\frac{1}{4}\frac{a^3}{r^3}\right\}+\frac{3\lambda}{4}\frac{a}{r^3}\left(1-\frac{a^2}{r^2}\right)x^2,\\ v&=\frac{3\lambda}{4}\frac{a}{r^3}\left(1-\frac{a^2}{r^2}\right)xy,\\ w&=\frac{3\lambda}{4}\frac{a}{r^3}\left(1-\frac{a^2}{r^2}\right)xz. \end{aligned}\right\} \quad . \quad (53)$$

For this case s reduces to

$$s=-\frac{3}{2}\frac{\lambda a}{r^3}x,$$

and consequently

$$\left.\begin{aligned} \xi&=0,\\ \eta&=\frac{3}{2}\frac{\lambda a}{r^3}z,\\ \zeta&=-\frac{3}{2}\frac{\lambda a}{r^3}y. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (54)$$

From these last equations we see that the vortex-lines are circles whose centres lie upon the axis of x. The function Θ was defined by the differential equations

$$\frac{d\Theta}{dx}=\frac{dP}{dx}-2\tau\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\text{ \&c.};$$

substituting in these the values of ξ, η, ζ, from (34) we obtain

$$\Theta-P=s-r\frac{ds}{dr},$$

which is easily seen to satisfy the equation

$$\nabla^2(\Theta-P)=0.$$

Substituting this value of $\Theta-P$ in equation (17), we have

for the stream-line $\int ds$,

$$p=\rho\left\{r\frac{ds}{dr}-s-V-\tfrac{1}{2}q^2\right\}+C.$$

It does not seem to me to be possible in this general case to determine the forms of the stream-lines, or the resistance which the friction of the fluid opposes to the motion of the sphere. In the case where the motion is symmetrical to the axis of x, the resistance has been determined by Lamb (' Treatise on the Motion of Fluids,' page 126) by means of the dissipation-function; that process would scarcely be applicable to such a complicated case as the one in hand. I do not see that any other results of importance can be obtained from further study of this problem, unless, indeed, the velocities u, v, w can be given in some other form than that of an infinite series.

Washington, June 12, 1880.

XLII. *On the Thermic and Optical Behaviour of Gases under the Influence of the Electric Discharge.* By E. WIEDEMANN*.

[Plate IX.]

1. *Introduction.*

IN two previous experimental investigations (Wied. *Ann.* v. p. 500, 1878, vi. p. 298, 1879) I have examined the luminosity of gases under the influence of the electric discharge. The result of the first investigation was, that when a mixture of two gases is exposed to the electric discharge, of which the one is a metallic vapour and the other nitrogen or hydrogen, the lines of the metallic vapour are seen in the spectrum, while those of the other gas remain invisible; so that the propagation of the electricity is due entirely to the metallic vapour. The discharge in it is entirely discontinuous. The same result has been recently obtained by H. W. Vogel†, by photographic methods.

The second investigation showed that the temperature of the gas illuminating a Geissler's tube may be below 100°.

This last result has been recently confirmed by Hasselberg‡. Hittorf also has shown, but without accurate measurements, that the same conclusion is throughout in agreement with the phenomena.

* Translated from Wiedemann's *Annalen*, No. 6, 1880, with additions and corrections by the Author.

† H. W. Vogel, *Beibl.* iv. p. 125, 1880.

‡ Hasselberg, *Beibl.* iv. p. 132, 1880.

It has been shown by these investigations that the usual theory, which refers the emission of light by a gas under the influence of the electric discharge to an increase of temperature up to the point of incandescence, is no longer tenable, and that this point requires fresh experimental examination.

Spectrum analysis, so far as it employs the electric discharge for the production of spectra, has to examine the question of the dependence of the spectra produced on the quantity of electricity transmitted. For this purpose we must pass discharges of given quantities of electricity through a given gas of definite pressure, observe the spectral phenomena, and at the same time determine the magnitude of the quantities of energy or of heat given off by the discharges; for these alone can furnish a measure of the resultant phenomena of motion in the gas. As we cannot deal with a single discharge, we must determine the number of discharges which correspond to a given quantity of electricity. It is absolutely necessary that the gas should return to its original condition before the entrance of each new discharge, so that any previous heating may have ceased to be sensible, and especially that the gas should have become dark again.

The present research is intended to be a first contribution to the solution of this unusually complicated question.

In the first place I have thoroughly examined the thermic relationships of the discharge of the induction-machine under different conditions, and in doing so have observed a peculiar behaviour of positive and negative electricity. Then follow experiments to determine numerically the conditions under which the transformation of the band spectrum into the line spectrum occurs in the case of hydrogen, as well as some contributions to our knowledge of the discharge taking place from the negative electrode in highly rarefied gases; then follows a discussion of the employment in spectrum analysis of the other sources of electricity—induction coils, large galvanic batteries, and Leyden jars, and of the continuous and discontinuous discharge in gases.

Theoretical considerations on the discharge in gases and on the nature of spectra form the conclusion. There are particular subjects more briefly treated in this first paper, which shall be more fully investigated later.

2. *Apparatus.*

In correspondence with the problems for investigation, the apparatus employed consisted of three parts :—(1) the source of the electricity, and the arrangements to measure the intensity of the current; (2) the discharge-tube and its electrodes,

the calorimeters, and arrangements for exhausting ; (3) the arrangements for measuring the number of discharges.

1. The source of electricity employed was usually a Töpler's * electrical machine with 20 revolving disks, which was driven by a small Schmidt's water motor. In order to have a constant stream of water, most of the experiments were performed at night. In this way a constant velocity of revolution, and a constant deflection of the galvanometer included in the circuit, could be maintained for hours together.

The machine has a few drawbacks, in consequence of the powerful evolution of electricity. First, it attracts the dust very largely, the disks become dirty, and the production of electricity diminishes or even ceases altogether ; moreover large quantities of ozone are produced. Both of these defects are remedied pretty well by covering the machine over with a wooden cover with glass sides, and placing vessels of linseed-oil under it † After use for several weeks the disks become dirty in spite of all precautions, and must then be washed, either with water or with alcohol, by means of a sponge. Simple brushing is of no use.

In damp weather, and when the disks have become dirty, the direction of the current frequently becomes reversed. This may be prevented tolerably completely by warming the machine. A further peculiarity of this machine is that almost always when it has been allowed to run for a time, then stopped, and again put into action, the direction of the current is the opposite to that obtained at first. This was often very convenient in my experiments, since the electrode connected with the machine was generally wanted to serve first as a positive, and then as a negative electrode.

In order to introduce sparks of known length into the circuit, a spark-micrometer with spheres of 33 millims. diameter was employed. If no spark was to be introduced, the horizontal adjustable brass bars which carry the spheres were connected by a brass stirrup.

The reversal of connexions was always made on the machine itself, copper wires covered with gutta-percha being used for the connexions. One pole of the machine was always connected direct to earth, the other connected to the one insulated electrode of the discharge-tube, the other electrode of which was connected with the earth through the galvanometer.

* Töpler, *Berl. Ber.* 1879, p. 950 *et seqq.*; *Beibl.* iv. p. 398, 1880.

† The use of turpentine is not to be recommended, as it gradually forms a sticky substance on the disks, which destroys the varnish. This sticky substance probably consists of oxidation-products of turpentine, as the disks themselves may be washed with turpentine without damage.

The galvanometer was a Wiedemann's reflecting galvanometer of moderate strength, with coils of gutta-percha covered copper wire. The distance between the telescope and scales was 189 centims.

2. The discharge-tube includes the space containing the two electrodes and the part of the tube uniting them. This either was altogether absent so that the electrodes were immediately opposed to each other, or consisted of tubes of various shapes. But care was taken to avoid any shape in which two portions of the tube would be parallel to each other, as, for example, in a U-tube. In this case induction-phenomena of a very disturbing character occur.

The electrodes were made of aluminium in all definitive experiments, since this metal alone is not subject to dissipation by the discharge. They were either balls of 5 millims. diameter, or points 3 millims. thick at the base and 12 millims. long.

In conducting the electricity to the electrodes, in order to avoid as much as possible the scattering by points, and to ensure good contact, the following arrangement was adopted (Plate IX. fig. 1) :—*a* is a glass-bulb, *b* the electrode, *c* a platinum wire screwed into it and covered with glass, *d* a glass tube joined to *a* by fusion, into which the wire *c* projects. The tube *d* can be filled with mercury, into which the wire conveying the electricity plunges. The discharge-tube is melted on at *m*. A similar arrangement is to be recommended for Geissler's tubes, which are much used, as they are easily broken by the pulling of the connecting wires at the projecting platinum wires.

If the heating was to be determined in the whole space between the two electrodes, the whole apparatus was plunged in a trough-shaped glass calorimeter of about 50 cubic centims. capacity. The arrangement shown in Plate IX. fig. 2 was employed to determine the heating at one electrode. *e* is the electrode with the portion of the tube surrounding it, *t* the thermometer, *c* the calorimeter with a tube-shaped portion *l* in which a glass tube *s* melted on to the discharge-tube was cemented. The part of the tube *s* next the electrode was constructed out of a capillary tube, in order as much as possible to prevent convection of heat by currents.

In order to determine the heating in the tube, the arrangements figs. 3 & 4 were employed. In the first, as in the experiment with the electrode, a portion *e* of the discharge-tube is carried vertically through the tube-shaped calorimeter *a*, and is bent horizontally above and below the calorimeter ; in the second the discharge-tube *e* is horizontal, and the calorimeter *a* is pushed over it. The last arrangement must be

employed if the heating is to be measured at points lying near each other of tubes of varying width, or if the thermic action is to be observed at one part of a tube and close to the spectroscopic phenomena.

The calorimetric fluid employed was turpentine. It has a small specific heat when equal volumes are compared, and is a good insulator, a point of special importance when wide tubes are employed. The calorimeters themselves were made of glass, after preliminary experiments with brass calorimeters had shown that the fraction of the energy of the gas lost by radiation was excessively small.

The calorimeters with the discharge-tubes in them were placed in a cylindrical metallic vessel with double walls, being introduced either simply from above, or through an opening in the bottom. In this way irregular currents of air &c. were excluded, and, besides, a trustworthy correction for loss and gain by radiation during the experiment was rendered possible.

With the horizontal tubes similar wooden boxes with openings at the sides were employed. The gas to be experimented upon was carefully dried and admitted into the completely exhausted tube. Only air and hydrogen were employed for the most part.

As air-pump, a Töpler's * pump was employed.

As its arrangement is not widely known, it may be briefly described, with a few small changes which I have made in it. A wide oval vessel A (Plate IX. fig. 5), about 15 centims. high and 10 centims. wide, has glass tubes melted on to it above and below. The upper one, a, is bent round immediately above the globe. Its diameter is about 2 millims.; and its length, reckoned from the highest point α, is somewhat more than the greatest usual barometric height. Its lower end plunges into a vessel L containing mercury: it is highly advisable to cut the end of the capillary tube in L obliquely. The lower tube b is about 1 centim. in diameter and 88 centims. long. About 6 centims. below its junction with the bulb, a side-tube B is attached, which is bent at right angles, and ascends to a height of about 760 millims. above α, where (at e) it is expanded into a wider cylinder for a length of about 8 centims., and is then bent down again; at the level of the bulb it is again bent at right angles, forming a horizontal portion at f, to which a vessel β, containing phosphoric anhydride, and a manometer γ are attached by grinding. A tap δ enables the whole pump to be shut off from the atmosphere. To facilitate the cleaning of the pump, the long tube B is made

* Töpler, *Dingl. Journ.* clxiii. pp. 426–432, 1862.

in two portions connected by a ground joint, the form of which is that recommended by Gimmingham *. The lower portion of the tube is provided with a funnel-shaped portion which is filled with mercury; so that if the joint becomes a little loose it is not necessary to stop working.

The lower end of the tube *b* is connected at *o* with one end of an india-rubber tube, the other end of which communicates with a vessel of mercury O, which can be raised or lowered by a pulley not shown in the drawing.

The action of the pump is extremely simple. The vessel O is first raised and mercury allowed to enter the bulb A, by which the communication between A and B is interrupted. The air is driven out of A, and escapes through the mercury in L. When a certain quantity has escaped, the vessel O is lowered. The air is now drawn out of B and the parts of the apparatus communicating with it, into the bulb A. This takes place in a somewhat violent manner, since large bubbles of air enter the exhausted space above A and throw the mercury about; so that too much air must not be driven out of A at first†. When the exhaustion has advanced somewhat, another precaution is necessary, since if the flask were raised too high the mercury might be driven violently into A, and might possibly cause fracture, a result less to be apprehended if the tube B is of sufficient width. The pump has the great advantage that it is constructed entirely without taps, it works very easily and surely, and gives a very good exhaustion. Tubes may easily be prepared by its means which show the phenomena discovered by Hittorf ‡ and recently further described by Crookes §.

The tubes to be exhausted were connected with the pump by means of a joint on the further side of the tap δ. In order to make connexion of the different pieces of apparatus with the pump, glass tubes of greater or less length must be employed connected with each other by joints. In order to make a secure joint, not liable to be affected by the shaking which occurred in pumping, the two parts *a* and *b* of the joint (Plate IX. fig. 6) were connected by an india-rubber band *e* passed round side-tubes *c* and *d*. Any accidental loosening was thus immediately remedied, and it was unnecessary to cover the joint with wax‖.

* Gimmingham, *Beibl.* i. p. 175, 1877.

† For this reason, the pump employed in the physical laboratory at Berlin has been modified so that the points Q and δ are connected by another tube. The shocks are therefore much less powerful.

‡ Hittorf, Pogg. *Ann.* cxxxvi. p. 8, 1869.

§ Crookes, *Beibl.* iii. p. 527, 1879.

‖ The whole of the glass apparatus employed was manufactured in an admirable manner by Mr. Götze, glass-blower in Leipsic.

3. For the determination of the number of discharges the very accurate heliometric method employed by Wiedemann and Rühlmann * could not be used, on account of the great number of discharges. It was difficult to attach a mirror to the axis of the machine itself, in consequence of the mode of its construction; and it made too few revolutions. In particular cases satisfactory results were obtained by a self-registering method. The end of the wire coming from the galvanometer usually connected to earth, was placed opposite to a cylinder covered with tin-foil and blackened; a tuning-fork was also made to record its vibrations on the blackened surface. The lamp-black is removed at each discharge. But this method is only applicable when the interpolation of a spark in the circuit is of no consequence.

The following method has been found satisfactory :—An ordinary Geissler's tube was placed in front of and parallel to the discharge-tube A, which was to be investigated thermically or spectroscopically. Both were then covered with paper so that the covered portion of the one corresponded to the uncovered portion of the other; and both were then observed in a revolving mirror. The discharge of a small induction-coil was passed through the Geissler's tube, the primary current of which was made and broken 100 times a second by means of a self-acting electromagnetic tuning-fork. The reflection in the revolving mirror showed a series of bright lines of light, at distances corresponding to hundredths of a second. By observing how many discharges of the tube A lay between two of the Geissler tubes, the number of discharges in $\frac{1}{100}$ of a second, and consequently the number per second, was ascertained. It was possible to make a tolerably accurate estimate when there were not more than 4 or 6 discharges in the $\frac{1}{100}$ of a second. But when the machine was in more vigorous action, as was always necessary when thermic measurements were to be made, the number was much greater (up to 60 times).

The number of discharges could, however, be obtained in this case by allowing the machine to run more slowly, and observing the deflection of the galvanometer which corresponded to a given number of discharges. In order to find the number of discharges during the measurements, it was only necessary to multiply the number observed in the first case by the ratio of the galvanometer-reading in the two cases. It is true that in doing so we make the assumption that the number of discharges is proportional to the current-strength; but this assumption is justified if between two

* Wiedemann and Rühlmann, Pogg. *Ann.* cxlv. p. 242, 1872

discharges the gas returns to its original condition—which is the case in our experiments, as no phosphorescence of the gas was observed.

This method, since it is one of estimation only, cannot of course give so accurate a result as the heliometric method; but it is sufficient for such qualitative-quantitative experiments as the present.

The measurements were made and calculated in the following way. The electrical machine was first set in action by turning on the water, the temperature of the thermometer in the calorimeter read off, and the galvanometer observed. The electrodes of the machine were then connected by a brass rod. After three to five minutes the temperature was read again, the brass rod removed by means of a cord passing over pulleys, and the current allowed to pass through the discharge-tube ; at the same time the galvanometer was observed. After an interval of from one to eight minutes, the machine was again closed, and the thermometer observed from minute to minute, and read off at the end of three minutes, when the movement of the temperature in the calorimeter had become uniform. The experiment was allowed to last so long that a change of temperature of two or more degrees resulted ; only in special cases, in which the heating in the whole tube, or that at the positive pole at very low pressures, was to be determined, was the change of temperature less.

After the experiment, the machine was stopped, and the calorimeter allowed to cool by radiation, so that its temperature might not alter too much. The measurements before and after the passage of the electricity enabled corrections to be applied to the observed rise of temperature, according to well-known methods. The correction could not be applied for the conduction of heat from the parts of the tube not surrounded by the calorimeter. Each experiment occupied at least half an hour.

The galvanometer-deflections were not reduced to angles, since for the degree of accuracy reached at present the error so caused may be neglected.

The heating is proportional, during varied action of the machine, to the deflection of the galvanometer, as my father's experiments and control-experiments of my own have shown. In fact, since the discharges are entirely discontinuous, and the intensity of each single discharge is the same, when n times as much electricity passes through there will be n times times as many discharges, and consequently n times as much heat produced.

I desire to express my best thanks to Mr. Roth for the assistance he has rendered me in the measurements.

3. *General Measurements.*

Three groups of observations were made, in order to obtain a general idea of the way in which the thermic phenomena of the tube depended on the pressure.

1. The heating in the whole of a discharge-tube was determined.

2. The heating in a capillary tube was investigated, and experiments made on the influence of the width of the tube and of the shape of the electrodes.

3. The heating at an electrode was determined when the electrode was positive and when it was negative, and also both when it was connected with the source of electricity and when it was put to earth.

In particular cases the number of discharges was determined, and the measurements made with hydrogen-vacua, and then repeated with air-vacua. Further, air-sparks were introduced into the circuit, always between the machine and the discharge-tube.

Further experiments were made on the influence of the condensation of electricity on the walls of the discharge-tube, using mercury instead of turpentine as calorimetric fluid.

The arrangement of the tables is always the same. The first column, headed p, gives the pressure. At very low pressures which could not be read on the manometer the degree of the exhaustion is denoted by x or xx.

The following columns give the corrected amounts of heating of the calorimeter in one minute, calculated for an intensity of current corresponding to a galvanometer-deflection of 100 millims.

$+$ or $-$ in the heading of a column indicates that the positive or negative electrode was connected with the machine, the other pole being put to earth.

The number of discharges given corresponds to a current giving a deflection of 10 millims.

I. *The heating in the whole tube* was determined by means of three different forms of apparatus.

The first consisted of a glass vessel, I, of the form shown in Plate IX. fig. 7. The distance of the electrodes was 16 millims., the water-equivalent of the whole apparatus 27·5 grammes.

Air.			Hydrogen.		
p	+	−	p	+	−
680	1·15	1·02	680	1·03	0·90
360	0·80	0·55	350	0·56	0·58
14·9	0·40	—	126	0·27	0·25
32·4	0·16	0·18	34·6	0·11	0·10
11	0·06	0·066	11·7	0·055	0·06
3·7	0·056	0·065	3·9	0·060	0·06
0·4	0·065	0·081	1·5	0·063	0·062
x	1·25	1·01	0·1	0·26	0·33
			x	0·85	0·50

The numbers show that, as the pressure diminishes, the quantities of heat evolved diminish to a minimum and then rise again. With hydrogen the heating is generally less than with air.

Hydrogen and air show with alteration of pressure the same change in the number of discharges. In general this was less in air than in hydrogen, and larger when the positive electrode was put to earth than when the negative electrode was so connected.

For example, hydrogen gave for the number of discharges:—

$$p=660,\ 45;\quad p=365,\ 75;\quad p=126,\ 180;\quad p=x,\ 50.$$

At pressures from 90 millims. to $\frac{1}{2}$ millim. the discharges were not to be separated, since a sufficient velocity of rotation could not be given to the mirror. Below the pressure of $\frac{1}{2}$ millim. the apparently continuous band of light resolved itself into separate luminous strips, the beginning and end of which were especially bright; but these strips also were resolved into single discharges as the exhaustion was continued.

The number of discharges increases therefore as the pressure diminishes, and then decreases again.

The measurement of the number of discharges is rendered very difficult at medium pressures by the extremely feeble intensity of the light, especially when the negative electrode is insulated. The colour of the conical discharge radiating from the positive electrode is greenish-white in hydrogen at a pressure of 365 millims.; a red spot appears at the positive electrode, at the point of the cone. When the exhaustion was carried far, the dark space round the negative electrode expanded as far as the positive pole, which it intersected in a circle. No positive discharge issued from within this space, which remained perfectly dark.

An increase in the number of discharges corresponds to a decrease in the quantity of electricity in each discharge, and consequently to a decrease in the potential before each discharge, if the electrodes remain of the same shape; but inas-

much as a decrease of the quantity of heat produced must result, the results found for the number of discharges and the quantity of heat produced are in agreement.

If the quantity of electricity e of potential V sinks to potential 0, the quantity of heat produced is proportional to eV. If, instead of one discharge of quantity e, n such discharges occur each of quantity $\frac{e}{n}$, then these sink from potential $\frac{V}{n}$ to potential 0. The quantities of heat produced are proportional to $n \cdot \frac{V}{n} \cdot \frac{e}{n} = \frac{1}{n} Ve$, and therefore only $\frac{1}{n}$ as great as in the first case.

Warren De La Rue and Hugo Müller * have also found a minimum value for the potential necessary to discharge with decreasing pressure, but only with air. The same conclusion may be drawn from the experiments of Morren and De La Rive† and others. Thus, for example, De La Rive found a maximum for the intensity of the induced current when the discharges from an inductorium were passed through a gas at continually decreasing pressure, and hence concluded that the resistance was a minimum. In the same way we may conclude there is a minimum of potential necessary to discharge—since the less electricity discharges itself backwards through the coil, and consequently the more passes through the gas, the smaller does the potential at the ends of the secondary coil become.

An exactly similar series of experiments was made with electrodes at a distance of only $1\frac{1}{2}$ millim. from each other in air. The water-equivalent was 28 gr. The following values were obtained:—

p	+	−
673	0·17	0·28
63	0·090	0·083
1·7	0·107	0·071
0·6	0·13	0·13
x	0·94	0·80

These numbers show the same result as the first series: *the heating at first decreases slowly, and then increases very rapidly as the pressure diminishes.*

The number of discharges was, for $p=0$, 90; for $p=63$ they were not to be counted; for $p=700$, about 250; so that here again we have a maximum.

* Warren De La Rue and Hugo Müller, Proc. Roy. Soc. xxix. p. 281 (1879).

† Morren and De La Rive, Wied. *Galv.* [2] ii. p. 316 &c.

That the amounts of heat here, especially at high pressures, are so much smaller than in the first case, is to be explained by the fact that the potential necessary for each discharge is here so much smaller than before, as the electrodes stood so much nearer to each other. The minimum appears here at a higher pressure than in the first case, since the decrease of heating which occurs in the spark and at the positive electrode is earlier compensated by the increase which takes place at the negative pole.

A third series of experiments is occupied with the determination of the total heating in hydrogen when between two ball-shaped electrodes is interposed a capillary tube about 1 millim. wide and 30 millims. long (Plate IX. fig. 8). In the first series the apparatus broke, in consequence of the high potential necessary when the pressure was very small, since the electrodes were somewhat too close together. The two series of observations are therefore not directly comparable. The first series gave:—

p	+	−
184	3·10	2·76
15	0·61	0·62
0·4	0·43	0·45

The second series, in which the water-equivalent was perceptibly higher, gave:—

p	+	−
1·3	0·134	0·15
x	0·73	0·48

Here also we see *that the total heating at first diminishes with decreasing pressure* (*first series*) *and then increases again* (*second series*).

It must remain for further experiments to decide in this case how positive and negative electricity behave, and what part is taken by the heating at the positive pole, and at the negative pole, and in the tube, and how these conditions alter when the length of the interposed capillary tube varies.

It seemed to be of interest to vary the quantity of electricity in each discharge, not only by change of pressure, but also *by interpolation of sparks*—and the more so since in the later experiments and in the study of the spectral phenomena, when the evolution of heat within the capillary tube was determined, this method became of manifold application.

With the apparatus I, in which only the quantity of turpen-

tine was somewhat different, the following amounts of heat were obtained for the positive discharge at the pressures p, when the spark-lengths 0 and 10 millims. were interposed:—

p	x	0·6	4·3	12·8	25·6	35	90	166·4	567	759
0	0·253	0·082	0·072	0·088	0·10	0·16	0·30	0·52	1·17	1·30
10	1·34	0·84	0·52	0·56	0·69	0·80	0·81	1·10	1·77	2·2

The number of discharges when no spark was interposed was

p ...	730	400	160	90	30
z ...	20	40	120	220	about 700

increasing now up to very low pressures and then decreasing.

With a spark-length of 10 millims. interposed the number z was about 13 for all pressures.

A comparison of these numbers shows *that the amounts of heat corresponding to the same quantity of electricity transmitted are by no means inversely proportional to the number of discharges.* Thus the number of discharges z_0 and z_{10}, and the quantities of heat w_0 and w_{10}, with the spark-distances 0 and 10 millims., and at the pressures p, are somewhat as follows:—

p	$z_0 : z_{10}$.	$w_0 : w_{10}$.
180	10 : 1	1 : 2
90	17 : 1	1 : 3
30	50 : 1	1 : 6

This may also be seen in another way. Whilst the amounts of heat between 759 and 4·3 millims. pressure without spark sink from 1·3 to 0·072, or to $\frac{1}{15}$, with spark interposed they sink only from 2·2 to 0·5—that is, only to $\frac{1}{4}$.

We may conclude from these facts that, when a spark is included in the circuit, the discharge of the whole electricity takes place at a lower potential than that which would result if the whole quantity of electricity passing in each single discharge were accumulated upon the electrodes. For then, if the number of discharges were increased ten times, the heat produced would become one tenth as great. The flow of electricity cannot, however, take place completely at the potential which is necessary for the commencement of the discharge, since then the heating would be independent of the number of discharges. We must rather assume that the discharge takes place at an intermediate potential, inasmuch as the electricity cannot flow off through the gas as fast as it flows in through the spark-length, and consequently becomes stored up.

A series of researches on the laws of the total heating of gases at high pressures when traversed by the discharge of a Leyden battery or of an induction-coil has recently been made by Villari*. They are in complete agreement with the consequences which follow from mechanical principles, as is the case with the problems treated by me. When, for example, Villari finds that the heating in sparks between two spheres is nearly proportional to the spark-length, that follows immediately from the law established by Macfarlane and others†, that the difference of potential necessary to the commencement of discharge between two spheres increases nearly proportionally to their distance apart, at any rate for short sparks. The same results were to be expected from the laws previously established connecting striking-distance with quantity of electricity discharged. Villari, however, is not able in his experiments to distinguish between the heating at the electrodes and in the spark itself, which corresponds to the capillary portion of a Geissler's tube.

II. Further, the *amounts of heating produced in the tubes connecting the electrodes were determined* when a spark of 10 millims. was included in the circuit, and when no spark was included. The width of the capillary tube (arrangement of fig. 3) was 0·4 millim.

The tables give the results obtained in a series of measurements.

Air.					Hydrogen.				
	+		−			+		−	
p	$F=0$	$F=10$	$F=0$	$F=10$	p	$F=0$	$F=10$	$F=0$	$F=10$
11·5	2·04	1·7	2·4	1·90	22	3·76	3·16*a*	3·45	3·2*a*
4·2	—	1·48	1·40	1·46	12·2	2·06	1·5	2·00	1·75
0·4	0·81	1·42	0·82	1·21	4·4	1·02	1·3	1·08	1·21
					0·6	0·49	1·16	0·50	0·92

a The spark measured only 5 millims.

To these observations belong determinations of the number of discharges. When the spark-length was 10 millims., the number reduced to a galvanometer-deflection 10 was always about 12 to 15 a second, whatever the pressure in the tube. The number decreased a little, it is true, with increase of pressure, within the limits of observation. It is otherwise when no spark is included in the circuit.

The results are given in the tables.

* Villari, *Beibl.* iii. p. 713, and iv. p. 404.
† Macfarlane, *Beibl.* iii. p. 429 (1879).

Air.			Hydrogen.		
p	+	−	*p*	+	−
17·5	40	80	24·4	35	55
6·2	75	90	8·2	100	86
2·4	100	90	3·1	80	101
1·1	75	110	0·8	90	350
0·3	40	180	*x*	55	170
x	60	140	*xx*	29	130

A second series with a somewhat wider capillary tube gave the following amounts of heat :—

Air.					Hydrogen.				
	+		=			+		−	
p	*F*=0	*F*=10	*F*=0	*F*=10	*p*	*F*=0	*F*=10	*F*=0	*F*=10
11	2·01	1·87	2·16	2·00	12·6	2·33	2·12	2·50	2·4
4·0	1·06	1·47	1·36	1·40	4·7	1·22	1·50	1·20	1·42
0·5	0·51	1·01	0·42	0·97	0·7	0·40	0·83	0·43	0·64
x	0·25	0·83	0·37	0·73	*x*	0·31	0·80	0·27	0·60

The corresponding number of discharges are :—

Air.			Hydrogen.		
p	+	−	*p*	+	−
14·0	66	33	13	80	90
5·9	83	60	5·1	90	90
2·2	150	110	1·1	90	120
0·8	110	190	0·3	60	280
0·3	50	330	*x*	60	90
x	50	200			
xx	30	90			

Here also the number of discharges was from 12 to 13 when a spark 10 millims. long was interposed.

With reference to the great differences of the number of discharges with and without interposed spark, the following general conclusion may be drawn from the observations :—

(1) *The heating in capillary tubes at pressures above* 1 *millim. is almost independent of the quantity of electricity passing in each discharge, and nearly proportional to the whole quantity of electricity which passes,* always provided that no Leyden jar or condenser is included in the circuit.

The deviations from this law at pressures under 1 milim., when the heating increases considerably when a spark is included, require further examination.

(2) *The heating produced by the positive discharge and by*

the negative discharge is nearly the same, in spite of the difference in the number of the discharges.

At a pressure of 0·7 millim., for example, the proportion of the number of positive discharges to that of the negative discharges is about as 1 : 4, the quantities of heat produced are about 0·40 and 0·43.

With decreasing pressure the heating diminishes rapidly, without passing through a minimum corresponding to the maximum number of discharges. A slight increase, however, was observed in some of the other tubes.

A special series of experiments had for their object to decide whether the heating in a capillary tube is dependent on the form of the electrodes or not. The arrangement of fig. 9 was employed. The measurements were made at several pressures, and with either positive or negative electrode put to earth, but without any spark included. The conducting-wires from the machine were plunged in c or d, the wires leading to the galvanometer in a or b.

The following table gives the amounts of heat produced :—

p	Spheres.		Points.	
	+	−	+	−
9·7	2·2	2·4	2·2	2·2
3·5	1·18	—	1·17	—
0·4	0·42	0·45	0·44	0·44
x	0·50	0·24	0·33	0·24

The amounts of heat, then, are in general nearly independent of the form of the electrodes.

This result agrees with that found when sparks are included.

Moreover the number of discharges alters very little with the form of the electrodes. Since somewhat long and wide portions of tube intervened between the electrodes and the capillary tube, the discharges were somewhat irregular, as they usually are in similar cases.

G. Wiedemann * has shown by experiments with Holtz's machine that the quantities of heat produced *in a wide tube and in a narrow tube* per unit length are always equal—a result which has since been confirmed by Naccari and Bellati † by use of the induction-coil.

I have, in the next place, made a further series of experiments with different thick-walled capillary tubes.

* Wiedemann, Pogg. *Ann.* clviii. p. 35, 1876.

† Naccari and Bellati, *Atti dell' Ist. Ven.* (5), vi. 1878; *Beibl.* ii. p. 720.

They were joined to each other by fusion, placed horizontally, and calorimeters pushed over them as shown in fig. 4. The narrower tube had a diameter of 0·6 millim., and the portion surrounded by the calorimeter a length of 42 millims.; for the wider tube the corresponding numbers were 2 millims. and 45 millims. The ratio of the sectional areas of the tubes was therefore 1 : 10; the water-equivalent was nearly the same for the two calorimeters.

The following table gives, in the usual way, the amounts of heat observed in the two calorimeters, which were always simultaneously observed:—

Hydrogen.					Air.				
p	$F=0$		$F=10$		p	$F=0$		$F=10$	
	wide	narrow	wide	narrow					
32·8	0·85	0·96	—	—	21·5	0·74	0·65	0·82	0·60
21	0·66	0·88	0·66	0·77	8·1	0·34	0·44	—	0·47
8·4	0·30	0·48	0·38	0·44	3·4	0·18	0·24	0·36	0·33
3·2	0·15	0·20	0·28	0·32					
0·6	0·06	0·17	0·12	0·24					
x	0·07	0·24	0·19	0·32					

The tables show (1) that, in agreement with the earlier results, *the difference of heating with and without spark is not very great*, both in wide and in narrow tubes; that, further, *the amounts of heat produced in the tubes of different width, with the same length of spark, do not differ very greatly.* In both cases, however, at low pressures there appear considerable deviations from these laws. In these, as in the former observations, the amount of heat increases perceptibly when the spark is included; and, moreover, the heating in the narrow tube is decidedly greater than in the wide one. Further insight into these relationships can only be obtained by additional experiments.

A further series of experiments was occupied with the behaviour of still wider tubes.

Two tubes, a capillary tube and a wider tube, of diameters 0·5 millim. and 4 millims., and thickness of glass 2 millims. and 1 millim., were united and included in the circuit, the calorimeters filled with turpentine, and the amounts of heat determined. The values obtained are given in the following tables. Those under A relate to the case in which the electrode next the capillary tube was put to earth, those under B to that in which the electrode next the wide tube was in connexion with the earth. The signs + and − mark the

nature of the electricity passing through, F the length of spark included ; *d*, *md*, *b*, and *mb* denote that the wide tube was dark, moderately dark, bright, or moderately bright.

A.					B.				
p	*F*		Heating.		*p*	*F*		Heating.	
			Narrow.	Wide.				Narrow.	Wide.
x	+	0	0·20	0·07	*x*	+	0	0·19	0·09
–	–	0	0·17	0·09	–	–	0	0·18	0·09
–	+	5	0·23	0·18	–	+	5	0·30	0·13
–	–	5	0·18	0·09	–	–	5	0·20	0·10
–	+	10	0·39	0·26	–	+	10	0·49	0·16
–	–	10	0·27	0·13	–	–	10	0·21	0·11
3·8	+	0	1·15	0·38 *md*	4	+	0	1·12	0·25 *d*
–	–	0	1·35	0·85 *b*	–	–	0	1·36	1·08 *b*
–	+	1	0·95	0·29 *d*	–	+	5	1·22	0·57 *b*
–	–	1	1·23	0·91 *b*	–	–	5	1·50	0·80 *b*
–	+	8	1·11	0·66 *d*	5·3	+	0	1·35	0·20 *d*
–	–	8	1·03	0·83 *b*	–	–	0	1·62	1·20 *b*
7·3	+	0	1·72	0·73 *d*	–	+	4	1·53	0·41 *mb*
–	–	0	2·25	1·33 *b*	–	–	4	1·76	1·14 *b*
–	+	5	1·85	0·63 *d*					
–	–	5	1·90	1·25 *b*					

These tables afford, in the first place, a confirmation of the former results, inasmuch as in narrow tubes the heating is very nearly independent of the quantity of electricity transmitted, especially at high pressures. Further, *deviations from this law occur, especially with the positive discharge.* In the wide tube they are perceptible optically as well as thermically. By including sparks, a wide tube may be made dark which was luminous before, whilst a still further increase of the spark-length calls forth again an increase in brightness. With this there goes hand in hand a diminution and subsequent increase in the heating of the calorimeters, as the observations at the pressure 3·8, for example, show. It is nearly indifferent whether the electrode nearest to the wide tube is connected with the source of electricity or with the earth.

This peculiar behaviour of positive electricity becomes much more marked when we use calorimeters with mercury instead of turpentine.

The following table gives a series of such comparative measurements, which were made at very low pressures, whilst the electrode nearest the wide tube was connected with the machine :—

F	+		−	
	Narrow.	Wide.	Narrow.	Wide.
0	0·23	0·19	0·164	0·13
0	0·37	0·89	0·18	0·12
4	0·39	1·44	0·27	0·16
8	0·40	2·72	0·40	0·12

Whilst during the passage of the negative discharge through the wide tube only relatively small changes in the heat produced occur with changes in the spark-length, these become very considerable (from 0·19 *to* 2·72*) when the positive discharge takes place through the wide tube.* At the same time there were always strong charges on the tube-wall, which led to a fracture of its upper edge at a somewhat high pressure in the tube. Further investigations on the influence of condensation of the electricity on the wall of the tube would probably lead to important conclusions as to the nature of the discharge. I shall return to this further on.

III. *The heating at the electrodes* was determined by means of the arrangement of fig. 9, and first when no spark was included.

In a first series the electrode for which the heating was to be determined was a point connected with the source of electricity. The gas employed was air.

The amounts of heat were as follows :—

p	+	−	p	+	−
14·4	0·50	0·74	1 2	0·08	0·48
6·2	0·48	0·39	0·45	0·07	0·74
2·4	0·17	0·34	x	0·06	1·26

In a second series the amount of heat was also determined with point electrodes, both when the electrode in question was connected with the machine and also when it was put to earth. The following table contains under *a* the values obtained in the first case, and under *b* those obtained in the second case. The water-equivalent of the calorimeter &c. was 6·4. The gas employed was hydrogen :—

a.			*b.*		
p	+	−	p	+	−
26·5	0·63	0·60	26	0·62	0·50
8·1	0·26	0·39	8	0·24	0·30
2·6	0·15	0·37	2·4	0·14	0·41
0·6	0·06	0·64	0·8	0·10	0·81
0·2	0·07	0·63	x	0·10	1·05
x	0·09	0·77	—	—	—

In a third series the electrodes were spheres, the gas was hydrogen; the water-equivalent was 7·4. The following numbers were obtained :—

p	*a.*		*b.*	
	+	−	+	−
31	0·94	0·93	0·96	0·83
2·7	0·227	0·42	0·20	0·40
0·2	0·11	1·31	0·064	1·33
x	0·21	1·66	0·36	2·40

The conclusions to be drawn from the tables are as follows :—

(1) *The heating at the positive electrode diminishes continually and rapidly as the pressure decreases. At very low pressures there is occasionally a small increase.* At the same time the glass round it shines with green light, as if the electrode became temporarily negative.

(2) *The heating at the negative electrode at first decreases as the pressure becomes less, and then increases rapidly.*

The heating results partly from the heating of the electrode itself, partly from that of the enclosed gas, and partly from that of the glass envelope. At quite low pressures, at the negative pole, where the glass becomes luminous, no doubt the last of these is the most important. I have calculated how great the heating of each square cubic centimetre of the wall would be under the conditions of the experiments, on the assumption that the whole of the heat at the end of the discharge issuing from the negative electrode is produced upon the glass wall. I chose for this purpose the observations with point electrodes given under *a*, at very low pressures. The luminous surface of the glass had a magnitude of 5 square centimetres; upon this surface there was produced in each minute a quantity of heat of $0·77 \times 6·4 = 4·9$ calories, or 0·98 per square centimetre. By way of comparison, it may be mentioned that the radiation of the sun causes each minute a production of heat on each square centimetre of the earth's surface amounting to about 2 calories. It is to be observed, however, that the discharge from the negative pole lasts only a very short time (not even the thousandth part of the whole time), whilst the radiation of the sun acts continuously for the whole time.

A large number of measurements with electrodes of different forms were made, to determine the influence upon the heating at the electrode of introducing air-sparks of varying

length; but the results were not very constant. A few of these observations are given in the following table.

−						+					
p	F=0.	2.	5.	10.	14.	p	F=0.	2.	5.	10.	14.
14·4	0·74	...	0·66	...	...	14·4	0·50	...	0·41	...	...
6·2	0·39	0·33	0·43	0·42	...	6·2	0·48	...	...	0·26	...
2·4	0·34	0·58	0·68	0·46	...	2·4	0·17	0·15	0·25	0·37	...
1·2	0·43	...	0·59	0·77	0·90	1·2	0·08	...	0·30	...	0·50
x	1·26	1·15	1·4	1·35	...	x	0·06	0·32	0·46	0·91	...

The constantly observed fact is to be particularly mentioned, that *at low pressures the heating at the positive electrode is unusually small when no spark is employed, but that it increases very rapidly as soon as a spark is introduced.*

We may bring together the results of the observations of the heat produced when no spark is included in the circuit as follows:—

(1) *The total heating first decreases, and then rapidly increases, as the pressure decreases.*

(2) *The heating of the tube decreases rapidly, and then increases very slightly.*

(3) *The heating at the positive electrode first decreases rapidly, and then increases slightly.*

(4) *The heating at the negative electrode first decreases slowly, and then increases rapidly.*

Hence it follows that, for the thermal phenomena in the whole discharge-tube, in the experiments which I have made, and under normal conditions without the use of any air-spark, the phenomena at the negative electrode are the most important.

But from experiments on the positive discharge it follows, further, that deviations from the laws stated above depend upon a peculiar behaviour (p. 374) of positive electricity, which becomes specially important, not when the passage of electricity through the tube takes place at the normal potential corresponding to the construction of the tube, but when by the use of sparks or external resistances the quantity of electricity passing in each discharge is increased.

4. *Absolute Determinations.*

In order to obtain a measure of the quantity of heat produced in the capillary tube per unit length by the unit quantity of electricity, special measurements were made.

The calorimeter was of glass, the fluid in it water. The length of the portion of the capillary tube surrounded by

the water was 9·4 centims., its diameter 0·58 millim. The water-equivalent of the calorimeter and water was 16·2. The elevations of temperature (reduced, as usual, to a galvanometer-deflection of 100 millims. and one minute) were:—

p	+	−
15·5	1·09	1·01
5·5	0·50	0·37

The mean number of discharges was 60,000 per minute when $p=15{\cdot}5$ millims., and 144,000 when $p=5{\cdot}5$ millims.

Next, the deflection of the galvanometer must be reduced to definite units. With a thermo-current produced by a copper-silver element whose points of junction were maintained at 13° and 29°·2, and resistances introduced of the values in Siemens's units 0, 1, 2, and 3 respectively, the galvanometer gave deflections of 122, 37, 21, and 15 millims. From these numbers, and from the distance of scale and mirror, viz. 189 centims., the resistance of the element and of the galvanometer is found to be 0·43 Siemens.

[The intensity of the current corresponding to a deflection of 100 millims. is 1·000336 Siemens-Daniell unit. The numbers given in the original paper are erroneous through a mistake in the determination of the galvanometer-constant. Accordingly the numbers in the foregoing part have been corrected. —E. W.]

If now we denote by t the rise in temperature of the calorimeter, W its water-equivalent, l the length of the portion of the capillary tube included, then the quantity of heat evolved per unit length $B=\frac{Wt}{l}$; and if we assume that this is proportional to the quantity of electricity transmitted e, we obtain for the quantity of heat X produced by unit quantity of electricity in unit time per unit length,

$$X=\frac{Wt}{le}.$$

The values of X in our experiments are as follows:—

p	+	−
15·5	3200	2966
5·1	2636	1962

in which the centimetre is the unit of length, the gramme the unit of weight, the minute the unit of time, and the unit quan-

tity of electricity that involved in the Siemens-Daniell unit of electromotive force.

The quantities of heat evolved are very large; consequently the electricity must suffer great loss of potential. This is also seen from the following consideration.

If in a Daniell's cell one equivalent of zinc (32·6 grammes) is dissolved, a quantity of heat amounting to 23,900 calories becomes free. A current of unit strength (Siemens-Daniell) would set free in one second 0·0116 milligramme of hydrogen or $0{\cdot}0116 \times 60 \times 32{\cdot}6 = 22{\cdot}7$ milligrammes zinc in one minute.

The whole quantity of heat produced in one minute by such a current is therefore $\frac{0{\cdot}0227}{32{\cdot}6} \times 23900 = 160$ calories, or about one twentieth of that found above.

The capillary tube had altogether a length of about 1 decimetre; so that in it alone, without taking account of the heating at the electrodes, about 45,000 calories would be produced by the unit quantity of electricity.

The temperature of the gas in the discharge-tube at each discharge may also be found from the experimental data, on the assumption that the whole heat goes to raise the temperature of the gas, and not to perform any internal work (see further on), and, further, that the discharge takes place so rapidly that there is no loss of heat externally.

If we denote by T the maximum temperature of the gas, c the specific heat * of unit weight, s the specific gravity at 760 millims, p the pressure at which the experiment was made, V the volume per unit length of the capillary tube, Z the number of discharges per minute, W the water-equivalence of the calorimeter, and t the elevation of temperature, we have

$$\frac{\mathrm{T} \,.\, c \,.\, s \,.\, p \,.\, \mathrm{V} \,.\, \mathrm{Z}}{760} = \mathrm{W}t.$$

According to the data of the experiment, the value of T was:—

p	+	−
15·5	1977	1830
5·1	1141	849·2

* The specific heat at constant pressure has been taken; for the heating of the gas in the vacuum-tube does not take place without expansion, as is shown by the following experiment. If a capillary tube be interposed between two wider tubes, then the whole be filled with hydrogen, and by interposing sparks the quantity of electricity be so far increased that the capillary tube assumes a bright-red colour, the red light continues a short distance (2–3 millims.) into the wider portion of the tube, as if the red luminous gas were driven into it.

Hence the temperature in these narrow tubes is already very low; in one ten times as wide it might sink to 100° C.

This is a further confirmation of the previously stated law that *a gas may be rendered luminous by electric discharges without any corresponding elevation of temperature.*

[To be continued.]

XLIII. *Air-Thermometers.* *By* D. Winstanley*.

[Plate VII.]

A THERMOMETER which makes its indications in consequence of the dilatation and contraction of a gas offers several advantages over one which depends therefore on the volumetric variations of a liquid. Gases under constant pressure expand considerably more than liquids do for the same elevation in their temperature. Hence an air- or gas-thermometer, having the same size of bulb and tube as one in which a liquid only is employed, will have a more legible and open scale. Again, a given volume of a gas at the ordinary barometric tension of the air upon the level of the sea, when compared with an equal volume of a liquid body, requires so utterly insignificant an amount of heat to elevate its temperature through a given range, that a gas-thermometer is enormously more sensitive than one which depends upon a liquid for its expansional effects. And, finally, the very equal manner in which gases are dilated under the influence of equal increments of heat is a very strong point in favour of an air-thermometer.

As constructed by Galileo, the air-thermometer unfortunately gave readings which were influenced by the barometric variations of the outer air, a circumstance which has limited considerably its use and application. Happily it is not difficult to construct an instrument which shall be free from this defect. If we take an ordinary mercurial barometer made after a certain well-known pattern, *i. e.* with a bulb-shaped cistern surmounted by a neck into which we may insert a cork, and if as a matter of fact we *do* insert a cork, obviously that barometer ceases to show the tension of the outer air, and is a barometer only to the air enclosed within its bulbous cistern. But as the tension of this air will vary with its temperature, the height of the mercurial column will vary therewith as well; and that which *was* a barometer will, by the mere insertion of a cork, have become an air-thermometer, the readings of which are uninfluenced by the

* Communicated by the Physical Society, having been read at the Meeting on June 26.

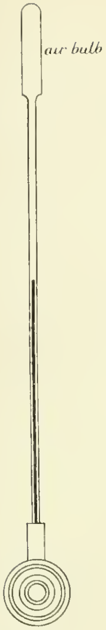


Fig. 1.
Air Thermometer

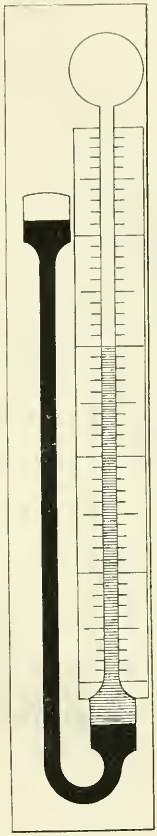


Fig. 2.
Air Thermometer.

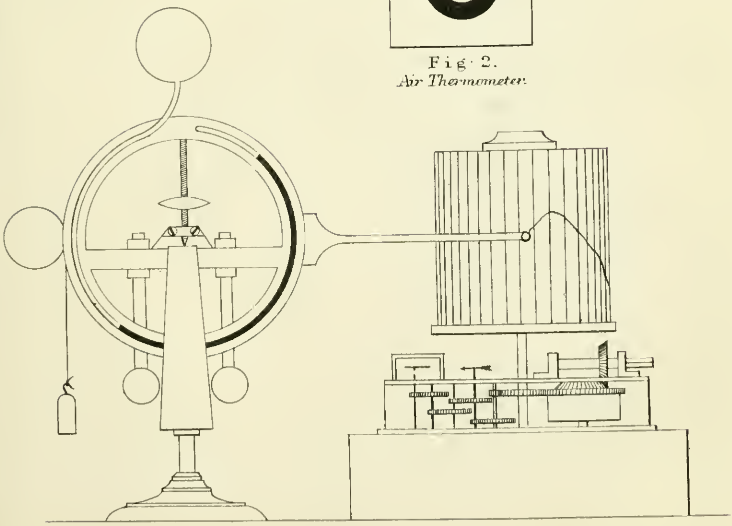


Fig. 3.
Thermograph.

barometric variations of the external air. There is, of course, no reason why the tube of such an instrument should be so little or so much as thirty odd inches long. Neither is it necessary that mercury should be the liquid used. Sulphuric acid, for instance, will answer just as well. It is not even needful that the liquid should be non-volatile at the temperatures to which the apparatus is exposed. The objection which exists to the employment of a volatile liquid in the ordinary barometric column does not here apply; for that depression which the vapour tends to produce in one limb it tends to produce in the other one as well, and so leaves the equilibrium of the liquid undisturbed, and dependent only on the expansion and contraction of the air. It is merely needful, then, in this form of air-thermometer, that the gas enclosed shall be submitted to the definite pressure of an isolated barometric column; and we may employ what liquid and what length of tube we please. We may even construct a veritable air-thermometer *without* a barometric column by resorting to some other means of obtaining the definite pressure we require; and to this end the author has employed a vacuous corrugated elastic box similar to those contained in aneroid barometers; but he has used it with the spring inside. This thermometer is shown in fig. 1. At the bottom we have a rigid box with the elastic one enclosed. The rigid one is sealed; and the elastic one is surrounded by a liquid which rises some distance up a tube, in which the indications will be made. The tube is surmounted by a bulb of air. This air, with a rise of temperature, expands and forces down the substance of the liquid column into the space made for it by the compression of the elastic box; and with a fall of temperature the reverse of course takes place.

The air-thermometer which I first described (that with the isolated barometric column) cannot, in the shape in which I have described it, be regarded as having a convenient form, inasmuch as through the greater portion of its length the liquid will never move. Obviously the index of the barometric column will not reach the level of the liquid in the cistern until the rational zero is attained; and the amount of space which marks the range between the freezing- and the boiling-points of water will have nearly three times as large a space below, devoted for the greater part to readings which will very likely never be observed.

I find, however, a very convenient instrument may be produced if, in its construction, two liquids are employed, as shown in fig. 2. There we have a mercurial barometric column. Its cistern is connected with the bulb of air by a,

long and upright tube. A column of sulphuric acid rests upon the mercury within; and the summit of the barometric tube is considerably enlarged. The tube connecting the barometer-cistern with the bulb of air is comparatively narrow in its bore. In consequence of this arrangement, the vertical amount of liquid motion is practically confined to the substance of the lighter column, and is several times greater in extent than if one liquid only had been used. We are accordingly enabled in this way to make an air-thermometer, of which the index shall move over pretty nearly its total length for such natural changes in temperature as we meet with in a given place. Such an instrument was constructed for me in Paris in 1878. It has hung in the Loan Collection of scientific instruments at South Kensington Museum for something like a year; and the accuracy of its indications does not seem impaired. The desirability of using coloured sulphuric acid as the material of the lighter column will very likely be a matter for dispute. I am aware that other experimentalists who have employed it in barometers have found a depression of the column has ensued and a crystalline deposit been left on the mercurial surface. It is for me, however, to speak as I have found. Twelve months ago my instrument, when laid down flat, had a bubble of air in the mercurial limb purposely introduced, and of obviously less volume than a small pin's head. The instrument has never been reversed to float it out, and the volume of this air is still capable of the same description as before. This thermometer is four feet or more in length; and the diameter of its index-column (which is cylindrical) is the tenth of an inch or so. Its Fahrenheit degrees are represented by spaces of the third part of an inch; and it attains an exceedingly close approximation to the actual temperature of the air in some few seconds' time; whilst an alcohol thermometer, moving an equal column through equal spaces for equal numbers of degrees, and with a bulb similar in shape but proportional in size, requires some hours to reach an equally close approximation to the temperature of the air.

The air-thermometers I have now described have depended for their indications on the movements of a liquid in a tube. I have, however, devised another, in which the movement of the tube about the liquid is the method I employ. In this thermometer (fig. 3) the barometric tube is circularly curved and mounted concentrically upon a wheel of brass, which is supported in a vertical position by a knife-edge of hardened steel which passes through its geometric centre and rests on agate planes. Adjustments are provided by means of which the centre of gravity of this arrangement can be raised or lowered; and the mercurial column which is seen extending

on the right is balanced by a weight, which is seen depending on the left. The whole is surmounted by the bulb of air; and the vacant space above the mercury upon the right is the Torricellian vacuum of the barometric column. Changes in temperature are shown by this thermometer in the varying angular positions of a needle, prolonged from one or other of the radii of the wheel, and counterpoised by a piece of metal on the other side. When the centre of gravity of this system has been made coincident with the point on which it turns, the liquid, under changes of temperature, is almost absolutely motionless, whilst the tube which holds it moves. By bringing the extremity of the needle into contact with a cylinder driven by clockwork at an even speed, we have a thermograph complete. Of course some delicate method of recording must be used; and I have hitherto employed the smoked-paper process, so much adopted in the observatories of France. I prefer to make my records on the albumenized paper prepared for photographic use, and, for the sake of the beauty of the black, to smoke it over the flame of a common tallow dip. I then fix the records by immersing, cylinder and all, in lac varnish diluted with methylated spirit. In this process there is not the slightest danger of injuring the results; and these, when several times revarnished and mounted upon card and rolled, are doubtless permanent, and are certainly incomparably more beautiful than any other tracings I have seen.

XLIV. *Notices respecting New Books.*

Catalogue of Books and Papers relating to Electricity, Magnetism, the Electric Telegraph, &c., including the Ronalds Library. Compiled by Sir Francis Ronalds, *F.R.S.* London: E. and F. N. Spon.

IN publishing this Catalogue the Society of Telegraphic Engineers have rendered invaluable assistance to those interested in following the development of the science of Electricity.

The Catalogue, with its 13,000 entries arranged in the alphabetical order of the authors' names, and annotated to some extent by the compiler, comprises nearly every contribution made to the science in English, French, German, and Italian up to the date of 1873; and we are glad to learn from the Preface that the Society contemplate completing the Catalogue by a Supplement.

Originally undertaken as material for writing a history of Electricity, the collection of the books and compilation of the Catalogue occupied Sir F. Ronalds during the latter years of his life; and it is to be hoped that the great labour thus voluntarily incurred may prove of some value to his successors. It was this hope that induced him to leave his Library and Catalogue to the late Mr. Samuel Carter, with the request that he would so dispose of it as best to

further the object he had in view. The Society of Telegraphic Engineers, in accepting the gift of the library, undertook to print the Catalogue, bind the books, and render them accessible to the public—all of which conditions, we believe, have now been fulfilled.

Mr. Alfred J. Frost, the Librarian to the Society, has appended a short biographical notice of Sir F. Ronalds *, including some passages from his own account (published in 1823) of the first working electric telegraph, erected by him in 1816. These passages are interesting as illustrations of the discouragement which inventors often have to encounter in bringing their discoveries into practical operation, and which were not wanting in the case of the electric telegraph, and also as historical evidence of the mean opinion held by men of eminence and authority half a century ago about an invention which is now found to be one of the most important instruments of modern civilization.

XLV. *Intelligence and Miscellaneous Articles.*

ON THE LAW OF ELECTROMAGNETIC MACHINES. BY J. JOUBERT.

I ASK of the Academy permission to submit some of the most remarkable results, all verified by experiment, which are deduced from the formula I had the honour of presenting at the last sitting.

That formula expresses the law of an important class of magneto-electric machines, characterized by the condition that the variations of the primitive magnetic field follow the law of sines. It gives

$$I = \frac{\frac{2E_0}{\pi}}{\left(R^2 + \frac{4\pi^2U^2}{T^2}\right)^{\frac{1}{2}}}$$

for the mean intensity of the elementary currents, and

$$\tan 2\pi\phi = \frac{2\pi U}{TR}$$

for their phase.

The theory indicates, and experiment verifies in the most rigorous manner, that the maximum value of the electromotive force E_0 during the course of a period is proportional to the velocity; if its value when the machine makes one revolution per second be called e_0, and if there are n periods per revolution, we can put, for a given intensity of the field,

$$E_0 = \frac{e_0}{nT}.$$

The formula of the mean intensity then becomes

$$I = \frac{2e_0}{n\pi(R^2T^2 + 4\pi^2U^2)^{\frac{1}{2}}}.$$

* Bringing prominently forward his early work in Telegraphy and Electricity, &c.

It is seen that this intensity does not increase indefinitely with the velocity, but tends towards a limit value

$$I = \frac{e_0}{n\pi^2 U},$$

which is besides very near those obtained for moderate velocities and weak resistances.

The expression of the total electrodynamic work of the machine, designating by I′ the square root of the mean of the squares of the intensities, is

$$W = RI'^2 = \frac{Re_0^2}{2n^2(R^2T^2 + 4\pi^2U^2)}.$$

This expression tends towards zero when the resistance increases indefinitely, and becomes 0 when the circuit is opened; experiment shows, in fact, that in this case we have no other work to overcome than that of the passive resistances*. But, contrary to what takes place with a pile, its maximum does not correspond to an external resistance $=0$. The work at first increases when the resistance is augmented, and passes through a maximum corresponding to the equation

$$RT = 2\pi U.$$

The conditions of the maximum of work can always (as is seen) be realized either for a given velocity or a given resistance; and it is always advantageous to work the machine under those conditions. The equation of the phase then gives

$$\tan 2\pi\phi = 1, \text{ or } \phi_m = \tfrac{1}{8},$$

that of the mean intensity

$$I_m = \frac{e_0}{n\sqrt{2\pi^2 U}},$$

and that of the maximum work

$$W_m = \frac{e_0^2}{8n^2\pi U}\,\frac{1}{T}.$$

Thus, for a given intensity of the field, whatever may be the other conditions under which the machine works, from the moment when it gives the maximum of work

The retardation is equal to $\frac{1}{8}$ of the entire period;
The intensity is constant and equal to the quotient by $\sqrt{2}$ of the absolute maximum of intensity;
The electromagnetic work is proportional to the velocity;
The velocity is in a constant proportion to the resistance.

In the Siemens machine (a machine with four foci) on which my experiments were made, $U = 0{\cdot}104$ and $n = 4$. When the inductor is excited by a current of 10 webers, $e_0 = 22{\cdot}56$ volts: the absolute maximum of intensity is equal to 6·1 webers; and the intensity corresponding to the maximum of work is equal to 4·31

* It is not the same if the movable system in the magnetic field includes metallic pieces of some extent, and particularly masses of soft iron.

webers. The maximum of work is $\frac{12\cdot 2}{98\,\mathrm{T}}$ kilogrammetres per second; lastly, the velocity which must be given to the machine to obtain this maximum is given by the equation

$$\mathrm{RT} = 2\pi \mathrm{U} = 0\cdot 653^{*}.$$

—*Comptes Rendus de l'Académie des Sciences*, Sept 13, 1880, t. xci. pp. 493–495.

ON THE ABSOLUTE MEASURE OF PELTIER'S PHENOMENON AT THE CONTACT OF A METAL AND ITS SOLUTION. BY E. BOUTY†.

Two coppered thermometers, as nearly equal as possible, and sensitive to $\frac{1}{200}°$, were lowered at the distance of 1 decim. from each other, into a large vessel full of solution of sulphate or nitrate of copper, which stood in a large cold-water bath. Thereupon the temperature of the solution changed in 5 minutes $\frac{1}{100}°$ only. Between the thermometers a current of the intensity i was conducted through during two minutes; the quantities of heat then observed on the two thermometers were proportional to $\mathrm{W} = \mp ai + bi^2$, where a and b are constants. That the indications of the thermometers were in fact proportional to the quantities of heat produced upon them follows from experiments, in which they were surrounded in the fluid with German-silver spirals protected from the latter. On the passage of currents i_1 through the spirals the heatings were proportional to i_1^2. In absolute measure (centim., gram, second) the heat generated, simultaneously with Peltier's phenomenon, by the current E in a second is $\Pi = \frac{\mathrm{T}}{\mathrm{A}}\frac{d\mathrm{E}}{d\mathrm{T}}$, where A is the mechanical heat-equivalent, T the absolute temperature, and E the thermoelectromotive force. According to previous experiments by Bouty (*Beiblätter*, iv. p. 680), for copper $\frac{d\mathrm{E}}{d\mathrm{T}} = 0\cdot 000696\,\mathrm{D} = 0\cdot 000696 \,.\, 1\cdot 12 \,.\, 10^8$ absolute units. Since $\mathrm{J} = 4\cdot 2 \,.\, 10^7$, it follows that $\Pi = 0\cdot 528$.

Further, the thermometer employed was heated in the liquid in 2 minutes $0°\cdot 471$, for which $4\cdot 77$ thermal units are requisite. From the formulæ for W it resulted that during 2 minutes $a = 6\cdot 018$. According to this, $\Pi = 6\cdot 018 \,.\, \frac{4\cdot 77}{0\cdot 471}\frac{1}{120} = 0\cdot 5078$, which agrees very well with the above value.

Similar results were given by salts of zinc and cadmium. Zinc in a solution of zinc chloride shows a constant thermoelectromotive force E in solutions the specific gravity of which is less than 1·6; with higher degrees of concentration E rapidly diminishes. Peltier's phenomenon Π behaves in just the same way. Thus,

Specific gravity	1·255	1·70	1·90	2·044
Constant E	1	0·724	0·247	0·053
Constant Π	1	0·709	0·244	0·051

* These experiments were made at the laboratory of the Société générale d'Electricité.

† *Comptes Rendus*, xc. pp. 987–990 (1880).

With other metals the determination is difficult in consequence of secondary processes.—Wiedemann's *Beiblätter*, 1880, No. 9, pp. 681, 682.

RESULTS OF PENDULUM EXPERIMENTS.

BY C. S. PEIRCE, ASSISTANT COAST AND GEODETIC SURVEY.

The following are the results obtained from observations made by me, for the U.S. Coast and Geodetic Survey, at four important stations, for the purpose of comparing the lengths of the seconds' pendulum, together with reductions to the sea-level and to the equator. In making the last reduction I have assumed the ellipticity to be =1 : 293, which is the latest result from measurements of arcs.

	At station.	At sea-level.	At equator.
	metre.	metre.	metre.
Hoboken	0·9932052	0·9932074	0·9910003
Paris	0·9939337	0·9939500	0·9910132
Berlin	0·9942399	0·9942482	0·9909865
Kew	0·9941776	0·9941790	0·9910083

The differences of the figures in the last column from 0·991 metre, a value conveniently near their mean, when reduced to oscillations per diem are:—Hoboken + 0·01^s; Paris +0·58^s; Berlin −0·59^s; Kew +0·36^s. The following are the residuals of former observations according to Clarke ('Geodesy,' p. 349):—

New York +0·20^s; Paris −3·29^s; Kew +2·89^s.

Colonel Clarke has used a value of the ellipticity =1 : 292·2, derived from pendulum experiments. This slight difference, however, is not important.

It should be explained that the result for Hoboken is derived from [T^2 Inv.] "Regular Set," given on page 318, and also on page 416 of the Report of the Superintendent of the U.S. Coast and Geodetic Survey for 1876. This number is treated as explained on page 319, where in the second line from the bottom for [T^2 Rev.] read [T^2 Inv.]. The altitude of the Hoboken station is stated on page 204. The numbers for the European stations are copied from page 320.

The length which I have taken as the metre has been derived from the German Eichungsamt, as fully explained in my Report. This is about 19·2 microns shorter than the quantity which is considered to be a metre in our own office of weights and measures, and is admitted in Berlin to be doubtful. It is impossible to fix the true metre at present; but I have but little doubt the above values will ultimately have to be diminished by about twenty microns on account of the error in the standard used.—Silliman's *American Journal*, October 1880.

ON THE ILLUMINATION OF ELECTRODES.

BY R. COLLEY*.

According to Slouguinoff (*Journ. de la Soc. phys. chim. de St. Pétersb.*), the light is intermittent which appears at electrodes

* *Journal de Physique*, 1880, ix. pp. 155–160.

(especially at a small negative one) immersed in liquids when the current is of considerable intensity.

According to Colley, in a rotating mirror a series of bright stars irregularly distributed upon a feebly illuminated ground are at the same time perceived; so that consequently the individual discharges issue from different parts of the electrode, while the time of the passage of the current is very short in comparison with that of the intermissions.

The spectrum at a negative electrode of platinum in dilute sulphuric acid shows, when 96 Bunsen elements are employed, at first the red (bright) and blue hydrogen-lines. In solutions of chloride of sodium and chloride of lithium the lines of those metals are seen in addition, and also (particularly five) platinum-lines are well seen.

Only by chance is the electrode herewith so strongly heated that the liquid no longer wets it. The passage of the spark cannot be owing to this, since the electrode may be quite cold and yet luminous. In order to prove this, Colley fixes in the neck of an inverted flask without a bottom a glass tube, in the upper end of which a platinum tube is cemented, and conducts a stream of cold water through. The flask is filled up to about 1 millim. above the upper extremity of the glass tube with dilute sulphuric acid, into which a cylinder of platinum foil dips. If the platinum tube, only a small surface of which is in contact with the sulphuric acid, serves as the negative, the foil as the positive electrode, yet the light appears. From this it follows that the high temperature really falls upon the liquid surrounding the electrode, which, on account of its small surface, opposes a great resistance to the current. As can easily be calculated, a current from 100 Bunsen cells could in fact readily heat to ebullition dilute sulphuric acid ($\frac{1}{5}$) close to an electrode of 10 square millims. surface, whereupon the passage of the sparks then ensues in the vapour, while the rest of the liquid and the electrode are only secondarily heated. Even when, in a narrow aperture, sparks are formed in the liquid itself, as Righi has observed, this arises from the formation of vapour there in consequence of the great resistance and the corresponding heating of the liquid.—Wiedemann's *Beiblätter*, 1880, No. 9, pp. 684, 685.

PHOTOGRAPHS OF THE NEBULA IN ORION.

BY PROF. HENRY DRAPER, M.D.

During the night of September 30, 1880, I succeeded in photographing the bright part of the nebula in Orion in the vicinity of the trapezium. The photographs show the mottled appearance of this region distinctly. They were taken by the aid of a triple objective of 11 inches aperture, made by Alvan Clark and Sons, and corrected especially for the photographic rays. The equatorial stand and driving-clock I constructed myself. The exposure was for fifty minutes. I intend at an early date to publish a detailed description of the negatives.—Silliman's *American Journal*, November 1880.

New York, October 2, 1880.

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[FIFTH SERIES.]

DECEMBER 1880.

XLVI. *The Disruptive Discharge of Electricity.* *By* A. Macfarlane, *M.A., D.Sc., F.R.S.E.**

[Plate XI.]

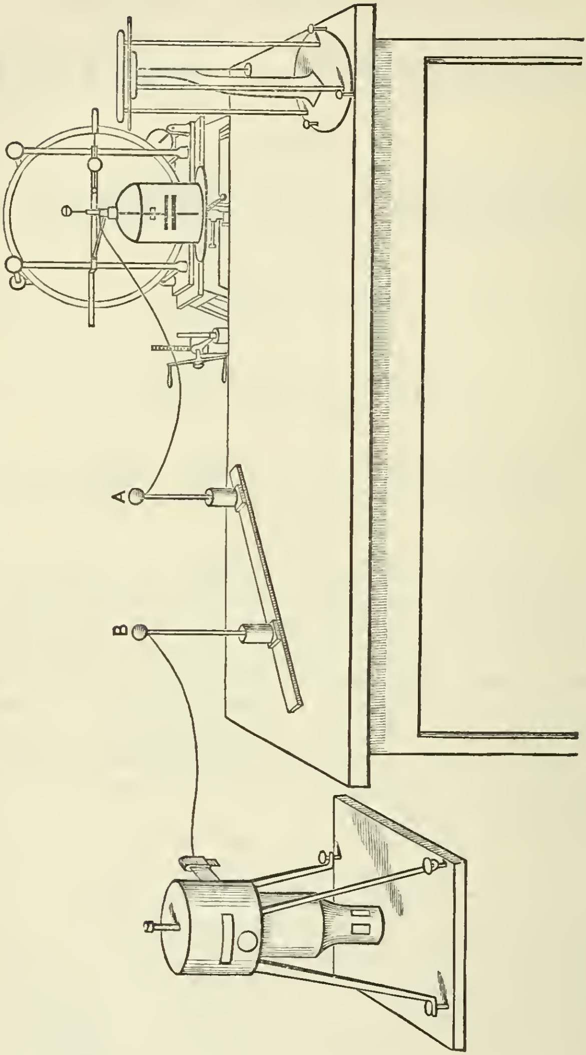
At intervals during the last five years I have carried on a research on the disruptive discharge of electricity; and the results obtained have been printed in the Transactions of the Royal Society of Edinburgh*. I propose in this paper to give a summary of the results. The experiments were made principally in the Physical Laboratory of the University of Edinburgh, where I had the ever-ready advice of Professor Tait; and in making the observations I had the assistance of several gentlemen, particularly of Mr. R. J. Simpson, M.A., and Mr. P. M. Playfair, M.A.

The method adopted can be readily explained with the help of the woodcut (fig. 1), which represents the apparatus *in situ*. The electricity was furnished by means of the Holtz machine, which had one conductor (generally the negative) always earthed by means of a wire, and the other conductor connected with the rod passing through the neck of the air-pump receiver. The diameter of the receiver is 19 centimetres. The electrodes were in general circular disks; and the one was screwed on to the rod mentioned, the other to a short rod normal to the plate of the air-pump, and in conducting connexion with its metal

* Communicated by the Author.

† Vol. xxviii. p. 633, and vol. xxix. p. 561; Proceedings, x. p. 555.

Fig. 1.



parts and the earth. A wire covered with gutta percha connected the insulated parts with the spherical ball A, which was supported on a varnished glass stem surrounded at the base by pumice-stones soaked in sulphuric acid. The equal and similarly insulated ball B was connected with the insulated half-ring of the Thomson's reflecting electrometer. The long-range absolute electrometer represented at the corner of the table was obtained on loan from Sir William Thomson, to reduce the results to absolute measure.

Thus an essential feature of the method, due to Professor Tait, is the introduction of the two insulated balls A and B, by the separation of which to a greater or less distance the effect of a charge upon the electrometer could be reduced to any convenient amount. By this means we have been able to measure very high potentials. When the potential of A is raised by working the machine, the potential of B is raised simultaneously; and this goes on until a discharge takes place between the electrodes inside the receiver. We found that it was possible, by breaking the contact between the conductors of the Holtz machine before beginning to turn the wheel, and by turning slowly and uniformly, to cause the wire-image to move along the scale continuously, and to be at rest at the instant of the discharge. After the passage of the discharge the image fell back to a position very near to the one it started from; and the coincidence was improved by discharging the couple of Leyden jars, which were generally attached to the conductors of the Holtz machine, for the purpose of making the charging more gradual. We noted the latter position of the index, and took the difference as proportional to the difference of potentials of the electrodes at the instant of discharge; for we found, by putting a charge on the ball A, dividing the charge by means of an equal ball, and so on, that the differences referred to were proportional to the amount of charge on A. Any escape of electricity from the charged conductor was at once made evident by the behaviour of the wire-image; it was this escape which put a limit to our measurements of potential, and not the range of the electrometer arrangement. In general we took three readings for each entry, so that we might be able, by taking their mean, to eliminate any error due to small variations in the mode of passing of the spark. A notion of the amount of this error may be obtained by inspecting the readings given below (p. 398). The method admits of so much rapidity, that we have taken a series of observations involving forty discharges in the course of an hour.

Measurement of the Difference of Potential required to pass a Spark through Air at the Atmospheric Pressure between Parallel Metal Plates at different Distances.

This problem, as is well known, was investigated by Sir William Thomson* for distances between ·0025 and ·15 centimetre. I have been able to extend the investigation to a distance of 1 centim. The plates used as electrodes were two equal circular brass disks of 10 centims. diameter. Both were rounded at the edge; and the lower one had its face slightly convex. The distance between the plates was measured by applying a glass millimetre-scale to a mark on the upper part of the rod. The shortest distance for which readings were taken was ·025 centim., and the greatest about 1 centim.; within that range the sparks were central and straight, while beyond it they passed in a curved line between the edges of the disks.

Five series of observations were taken, and were found to agree well with one another. One of them is plotted on diagram 1, Plate XI. The curve drawn through the points of observation closely resembles a hyperbola. I drew the asymptote which appeared to be indicated, found the equation of the corresponding hyperbola, and corrected that equation by a second application of the graphic method. Thus in one case the corrected equation is

$$V=(66{\cdot}69-3{\cdot}7142\,s)\sqrt{(s^2+{\cdot}2s)},$$

which gives

$$V^2=889{\cdot}52\,s+4348{\cdot}5\,s^2-494{\cdot}68\,s^3+3{\cdot}7142\,s^4,$$

where V denotes the difference of potential, and s the length of the spark. For s less than 1 the third and fourth terms are small; they are probably due to the finite nature of the plates. By neglecting them we obtain a hyperbolic function for V which agrees with the curve of observation at the beginning, and is only slightly greater at the end. By this treatment of the five series of observations I obtain the following results:—

Series.	Function for V.	*a*.	*b*.
I.	$66{\cdot}69\sqrt{s^2+{\cdot}2039\,s}$	·1020 centim.	6·800 C.G.S. units.
II.	$66{\cdot}51\sqrt{s^2+{\cdot}2030\,s}$	·1015 „	6·752 „
III.	$67{\cdot}34\sqrt{s^2+{\cdot}2035\,s}$	·1018 „	6·852 „
IV.	$65{\cdot}95\sqrt{s^2+{\cdot}2046\,s}$	·1023 „	6·745 „
V.	$68{\cdot}18\sqrt{s^2+{\cdot}2102\,s}$	·1051 „	7·163 „
Mean	$66{\cdot}94\sqrt{s^2+{\cdot}205\,s}$	·1025 „	6·862 „

* Papers on Electrostatics and Magnetism, p. 247.

The curve given by the mean equation is drawn on Plate XI. diagram 1.

Here a and b are the semiaxes of the hyperbola represented by the equation. The value of a is independent of the absolute value of the entries; and as its different values agree well with one another, we have here doubtless an important physical constant. I have also found its manner of dependence on certain conditions.

When hydrogen was substituted for air, and sparks taken through it at the atmospheric pressure, I obtained a series of readings which yield a result similar to that for air, namely

$$V=43{\cdot}19\sqrt{\{s^2+{\cdot}1369s\}},$$

giving $a={\cdot}0684$ centim. and $b=2{\cdot}954$ C.G.S. units. Thus a for hydrogen is less than a for air, the ratio being ·66, which is very nearly the ratio of the $\frac{b}{a}$ of the two curves (·65).

When sparks were taken through air at the reduced pressure of 180 millims., the curve obtained was

$$V=18{\cdot}29\sqrt{s^2+{\cdot}5253s},$$

giving $a={\cdot}2616$ centim. and $b=4{\cdot}785$ C.G.S. units. The value of a was always found to be greater the smaller the pressure. The distance at which the spark was first observed to pass between the edges was 1·65 centim., compared with 1 centim. for the ordinary pressure.

We found that it was possible to observe not only the maximum deflection just before discharge, but also the deflection corresponding to a continued discharge produced by turning the plates rapidly. The deflection was always less than that for the corresponding single discharge; and the zero was more displaced on the negative side of the original zero. The wire-image can be made to remain very steady, with only a slight oscillation. A discharge of this kind passed through air at the atmospheric pressure gave a curve

$$V=45{\cdot}58\sqrt{s^2+{\cdot}2046s},$$

which is similar to that for the single discharge; for the value of a is the same, the only difference being in the diminished value of $\frac{b}{a}$. Part of this diminution may be due to the variation of the potential of the charged conductor; but I attribute a considerable portion to a facility produced by the passage of previous sparks.

Having found the function for the difference of potential V, we can deduce that for the electrostatic force R, and that for

the electric tension p', which figures so prominently in Clerk-Maxwell's theory. For air at the atmospheric pressure we found

$$V=66{\cdot}94\sqrt{\{s^2+{\cdot}205\,s\}};$$

hence

$$R=66{\cdot}94\sqrt{\left\{1+\frac{{\cdot}205}{s}\right\}},$$

and

$$p'=\frac{(66{\cdot}94)^2}{8\pi}\left\{1+\frac{{\cdot}205}{s}\right\}.$$

On diagram 2 I have drawn the above curve for the electrostatic force, and also the curve deduced from Sir William Thomson's results. The electrostatic force is not constant, as the mathematical theory of electricity in equilibrium would lead us to expect; but it tends to become constant when the length of the spark is increased. The fact that the ratio of the a to the $\frac{b}{a}$ of the hydrogen-curve is equal to the corresponding ratio for the air-curve, points to the gas as the cause of the anomaly. By substituting a liquid for the gaseous dielectric, I have been able to show that the anomaly depends on the characteristics of a gas as compared with those of a liquid; and I shall adduce some experiments which were designed to test whether one of these differential characteristics is, as Clerk-Maxwell* suggested, a condensation of the gas on the surface of the electrodes. The following table gives the numerical values:—

Length of spark, in centimetres, s.	Difference of potential in C.G.S. units.	Electrostatic force, $R=\frac{V}{s}$.	Electric tension, $p'=\frac{KR^2}{8\pi}$.
·025	5·08	203·1	1322
·05	7·56	151·2	910
·075	9·70	129·4	666
·1	11·69	116·9	544
·2	19·05	95·3	361
·3	26·06	86·9	300
·4	32·93	82·3	270
·5	39·75	79·5	251
·6	46·52	77·5	239
·7	53·28	76·1	230
·8	60·02	75·0	224
·9	66·76	74·2	219
1·0	73·48	73·5	215

* 'Electricity and Magnetism,' vol. i. p. 56.

Measurement of the Difference of Potential required to produce the same length of Spark at different pressures of a Gaseous Dielectric.

We took four series of readings for a ·5-centimetre spark through air between the parallel disks above mentioned, varying the pressure from 20 millims. of mercury to the atmospheric. On diagram 3 I have plotted two of these: the upper curve was first obtained with the capacity of the conductor increased by the couple of Leyden jars; and the lower was obtained immediately afterwards with the jars off. Each of the series gave an experimental curve which coincided with a hyperbola, and more closely than in the case of the former set of curves with variable distance. The results are as follows (where p denotes the pressure, in millimetres of mercury):—

Series.	Function for V.	a.	b.
I.	$\cdot 04798 \sqrt{\{p^2+205\cdot 6p\}}$	102·8 mm.	4·668 C.G.S. units.
II.	$\cdot 04455 \sqrt{\{p^2+200p\}}$	100·0 "	4·455 "
III.	$\cdot 04634 \sqrt{\{p^2+199p\}}$	99·5 "	4·611 "
IV.	$\cdot 04685 \sqrt{\{p^2+207p\}}$	103·5 "	4·851 "
Mean	$\cdot 04579 \sqrt{\{p^2+202\cdot 9p\}}$	101·5 "	4·646 "

Here a is the semi- transverse and b the semi- conjugate axis of the hyperbola. The four values of a agree well with one another. To find out how it depended on certain conditions, I took several series of readings with change of a constant. With the length of spark increased to 1 centim. the equation is

$$V=\cdot 08062 \sqrt{\{p^2+219\cdot 8p\}},$$

giving $a=109\cdot 9$ millims. and $b=8\cdot 86$ C.G.S. units. The increase in the value of a, though small, is so decided that we may infer that the greater the length of the spark the greater is the value of a.

With hydrogen substituted for air, the curve obtained was

$$V=\cdot 024 \sqrt{\{p^2+600p\}},$$

which gives $a=300$ millims. and $b=7\cdot 2$ C.G.S. units. Thus, the length of the spark being ·5 centim., the value of a for hydrogen is thrice that for air.

When taking the fourth of the above sets of observations, at the lower pressures we observed not only the reading for

the single discharge, but also that for the continued discharge. As in the case of variable distance, so here, the equation of the curve is similar to that for the single discharge, and is

$$V = \cdot 03503 \sqrt{\{p^2 + 205\cdot 6p\}},$$

giving $a = 102\cdot 8$ millims. and $b = 3\cdot 602$ C.G.S. units. Here the ratio of the value of the $\frac{b}{a}$ for the continued discharge to that for the single discharge is ·75; in the former comparison it is ·69.

For a ·5-centim. spark through air we have

$$V = \cdot 04579 \sqrt{\{p^2 + 203p\}};$$

hence

$$R = \cdot 02289 \sqrt{\{p^2 + 203p\}},$$

and

$$p' = \cdot 00002085\{p^2 + 203p\}.$$

The above equation evidently does not hold for pressures lower than that for the minimum electric strength; it is certainly true for the range between that point and the atmospheric pressure; for Messrs. De La Rue and Müller* have obtained the same result; and I find that Prof. Röntgen's† numbers give the difference of potential equal to a hyperbolic function plus a constant. The existence of this constant may be due to the fact that the discharge was taken between a point and a plate, and was convective in nature. The hyperbolic law is probably true up to a limit much greater than the atmospheric pressure.

By expanding the function for V, first in descending powers of p and secondly in ascending, we obtain

$$V = A\left\{p + \frac{203}{2} - \frac{1}{8}\frac{(203)^2}{p} + \ldots\right\}$$

and

$$V = A\sqrt{203}\left\{p + \frac{1}{2}\frac{1}{203}p^{\frac{3}{2}} - \ldots\right\}.$$

Hence when p is large compared with 203, we have

$$V = A\{p + 101\},$$

which agrees pretty well with Knockenhauer's‡ formula, namely

$$V = 1\cdot 406\{p + 61\cdot 2\}.$$

When the third term is added, we obtain the formula which agrees best in form with that of Wiedemann and Rühlmann§.

* Phil. Trans. vol. clxxi. p. 78.

† Phil. Mag. Dec. 1878, p. 443.

‡ Mascart's *Electricité*, t. ii. p. 95.

§ Pogg. *Ann.* cxlv. p. 253.

When p is small compared with 203, we have

$$V = A\sqrt{203}\sqrt{p};$$

which agrees with what the conclusion of Masson* leads to when corrected by means of the law regulating the discharge between two balls (v. *infrà*, p. 402).

Messrs. De La Rue and Müller † have investigated, in the case of hydrogen, the nature of the curve for pressures lower than that of minimum electric strength (·6 millim.). The difference of potential is inversely proportional to the cube root of the distance. Assuming that the discharge through their long tube (33 inches long and 2 inches diameter) and between a ring and a point electrodes is similar to that between our disks at ·5 centim. apart, I obtain for the complement of the equation $V = {\cdot}024\sqrt{\{p^2 + 600p\}}$ the following,

$$V = {\cdot}67\frac{1}{\sqrt[3]{p}},$$

as the equation which is true for pressures less than ·6 millim. Let N denote the number of molecules in unit of volume; then when p is less than ·6 millim., V is inversely proportional to $\sqrt[3]{N}$; when p is greater than ·6 millim. but less than 600, V is directly proportional to $\sqrt{N}$; and when p is greater than 600 millims., V is directly proportional to N. Thus, on the one side of ·6 millim. the greater the number of molecules the greater is the facility for the discharge; while on the other side the greater the number of molecules the greater is the resistance to the discharge. This appears to be a strong argument in favour of Mr. Crookes's contention for a fourth state of matter; and the pressure ·6 millim. seems, in the case of hydrogen, to separate the two states. It is probable that the mode of discharge is different in the two regions.

Effect of Changing the Capacity of the Conductor.

The first two series of observations for varying pressure indicate that a change of the capacity of the charged conductor has little effect on the difference of potential required to produce the discharge between the disks. But to test this question further, I took readings for a ·5-centim. spark through air at the atmospheric pressure, with the capacity of the charged conductor, 1st, increased by means of a large Leyden jar, 2nd, increased by means of the couple of small Leyden jars of the Holtz, and, 3rd, without being increased. I append

* *Ann. de Chim. et de Phys.* 3rd ser. t. xxx. p. 41.

† Phil. Trans. vol. clxxi. p. 65.

the record of observations in full; both for the sake of the question considered, and also to give a sample of the observations made by the method.

	Deflection.	Zero.	Difference of potential.	Mean difference of potential.
Large jar	300	500	200	203
	295	496	201	
	290	490	200	
	293	486	193	
	296	505	209	
	297	,,	208	
	296	,,	209	
Couple of small jars.	298	505	207	207
	300	,,	205	
	296	,,	209	
	300	,,	205	
	296	,,	209	
Without any jar ...	315	510	195	209
	295	,,	215	
	310	,,	200	
	290	,,	220	
	290	,,	220	
	305	,,	205	
	300	,,	210	

If the last three readings for the large jar are, as is very probable, more correct than their predecessors, then the change of capacity has no effect upon the readings, provided the index of the electrometer is at rest at the moment of the discharge.

Effect of Heating the Electrodes upon the Passage of the Spark.

When both disks were heated before a fire before being screwed on, and sparks were taken immediately afterwards for varying distance, we obtained a series of readings which give a curve less bent at the beginning. It satisfies the parabolic equation

$$V=87{\cdot}04\,s-19{\cdot}56\,s^2.$$

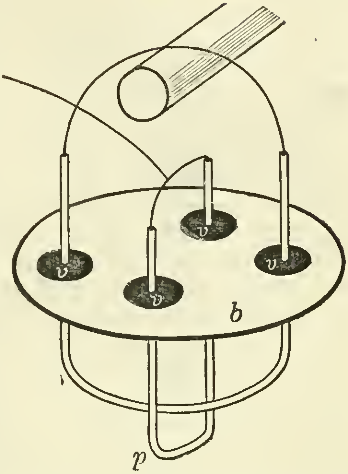
The diminution of the differences of potential for the smaller sparks must have been due directly to the higher temperature of the disks, or else to an alteration of the state of their surfaces brought about by the heating. A similar result was obtained for hydrogen.

To investigate this effect more minutely, I constructed a modification of an arrangement suggested to me by Clerk Maxwell for the purpose, and which was similar to that adopted by Sir W. R. Grove*. Two pieces of stout platinum

* Roy. Inst. Proc. 1854.

wire *p p* (fig. 2) were insulated in a brass plate *b* by means of vulcanite plugs *v*, and placed so as to be in planes at right angles with one another and with the brass plate. The shortest distance between the wires was 4 millims. The figure represents the apparatus suspended by means of the upper wire from one conductor of the Holtz machine, and the lower wire connected with the positive conductor. Only the wire which at the time was not bearing the weight could be heated without changing the distance of the wires. The wire operated on was made red-hot by bringing the terminals of a battery of four Bunsen's elements into contact with it at two points one inch apart; and the charging was made as soon as possible after the heating. Before heating, the spark required a difference of potential of 125; after heating, a mean difference of potential of 93. This was in the case of a single spark. When a continued spark was taken, a similar diminution of the deflection was observed. Here the effect could not be due to a rarefaction of the air between; it must have been due to something at the surface of the electrodes; and from the transient nature of the effect, I am inclined to ascribe it to the temperature of the surface directly.

Fig. 2.

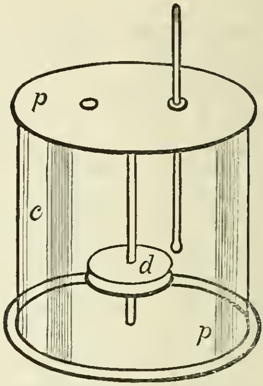


Measurement of the Difference of Potential required to pass a Spark through Air at different Temperatures, the Pressure being constant.

To investigate this question, I constructed the apparatus in fig. 3. A glass cylinder *c* was fitted into two brass plates *p* by means of grooves. The brass disks *d*, which were those used in the previous experiments, were screwed on, the one to a brass rod rising from the lower plate, the other to a rod moving inside a tube fixed to the upper plate; and they were set at a distance of ·9 centim. apart. The upper plate contains an orifice to allow the air to escape when the lower plate is heated, and a hole for the insertion of the thermometer. The lower plate was put to earth, and the

upper one charged by being pressed firmly against a projecting conductor of the Holtz machine. The heating was effected by means of a powerful Bunsen burner placed below the lower plate, and was carried on till the plate became red-hot. The thermometer indicated a range from 25° C. to 245° C.; and sparks were taken at each interval of 10° during both heating and cooling. The deflections are represented on Plate XI. diag. 4. The ordinates of the cooling-curve are less than the corresponding ordinates of the heating-curve—a result confirmed by two other series of observations. The falling off cannot have been entirely due to a leaking of the charge of the electrometer during the time occupied by the observations; for readings taken after the lapse of twenty-four hours showed that not more than one third of the difference could be so accounted for. The bulb of the thermometer being in the position indicated by the figure, gives the temperature of the air between the disks: hence it is probable that the effect was mainly due to the temperature or other state of the disks.

Fig. 3.



These curves also throw some light upon the question whether the difference of potential required to effect a discharge depends upon the temperature of the gas only if the change of temperature cause change of density. I have calculated the density of the air between the disks at the different temperatures, and plotted the difference of potential as ordinate to the density as abscissa. The curve obtained should coincide approximately with a portion of a hyperbola if the above question is to be answered in the affirmative. I find that it is convex towards the axis of abscissæ, and that it does not look like passing through the origin.

The Discharge through Liquid Dielectrics.

On proceeding to investigate the discharge through insulating liquids, we first took up oil of turpentine. The liquid was placed in a glass vessel of 7 inches diameter and 5 inches height. A screw passing through the bottom of the vessel served to fix the lower electrode, and also to connect it with the earth. The upper electrode was held immersed in the liquid by means of the rod of the receiver. We observed four

modes of discharge—by means of threads of solid particles, by motion of the liquid, by a disruptive discharge, and by motion of gas-bubbles. When a chain of particles was formed, the index of the electrometer indicated the passage of a current, which, when sufficiently great, broke the thread and turned into a spark-discharge. The liquid was more easily set in motion when its surface was not much above the upper plate. The bubbles of gas were formed by the passage of the spark. A disk, when used as the upper electrode, prevented them from escaping; and we observed that they were more strongly repelled when the disk was electrified positively than when negatively. At a diminished pressure they were observed to effect the discharge by carrying the electricity with them to the negative electrode. The bubble, when freely suspended, was not spherical, but had its axis in the direction of the line of force elongated. Similar phenomena were observed in paraffin oil, a liquid paraffin, and olive oil.

Measurement of the Difference of Potential required to pass a Spark through a Liquid Dielectric at the Atmospheric Pressure between Parallel Metal Plates at different Distances.

We have investigated this experimental problem in the cases of paraffin oil, oil of turpentine, and olive oil, for a length of spark ranging from ·1 to ·4 centim. and, in the case of the last, ·5 centim. Two precautions were requisite in making the observations:—one, to filter the liquid before beginning, so as to get rid of all solid particles; and the other, to eliminate the gas-bubbles produced by one spark before observing for its successor. It was impossible to carry the series of readings further than the above limits, because at the next greater distance the electricity escaped from the conductor and caused the image to be driven off the scale before the discharge through the liquid took place. In each case the readings gave a straight line, passing not exactly through the origin, but having a small negative intercept on the axis of ordinates.

Liquid.	Function for V.
Paraffin oil	$306{\cdot}4\,s-6{\cdot}1$
Oil of turpentine	$332{\cdot}4\,s-7{\cdot}2$
Olive oil....................	$298\,s-10$

The observations for paraffin oil are plotted on diag. 5. Thus a liquid dielectric has the electrostatic force and the electric tension both sensibly constant.

Measurement of the Difference of Potential required to pass a Discharge through Air at the Atmospheric Pressure between two Balls, between a Ball and a Plate, and between a Point and a Plate.

In the first case the electrodes were two spherical balls, each 1·5 centim. in diameter; and they were attached in the same manner as the disks. The curve obtained (diag. 6) coincides at the beginning with a parabola, but is afterwards bent in towards the axis of abscissæ. It is quite similar to that obtained by Mascart under the same conditions*. When the distance was 8 centims., violet sparks began to precede the white spark, and the electrometer simultaneously began to indicate the occurrence of a small discharge before the complete discharge. This caused the reading to be somewhat more ambiguous. At 10 centims. the electricity began to escape from the insulated wire.

On the following day a series of readings for the second pair of electrodes was obtained, and without any change in the arrangement other than the substitution for the lower ball of a circular plate 16·2 centims. in diameter. The plate was uninsulated, and the upper ball charged positively. The curve is drawn on the same diagram with that for the two balls; it also coincides with a parabola at the beginning, but at the greater distances is still more bent towards the axis of abscissæ. Violet sparks (giving an incomplete discharge) began much sooner, at 3·5 centims. compared with 8 centims.

A series of readings for the third pair of electrodes was obtained on the third day. A conical brass point was substituted for the upper ball. With the increased capacity due to the couple of Leyden jars the readings were regular; without it they were irregular. Up to 5 millims. the discharge was effected by a spark; but for greater distances there was only a faint brush or glow at the point, accompanied by a partial discharge.

Comparison of the Positive and Negative Electric Discharge.

To complete the preceding investigation, it is necessary in the second and third cases to take readings for several distances, with the ball or the point charged first with the one and then with the other kind of electricity. This I have been able to do; but as it was in the United College, St. Andrews, the apparatus was somewhat different. Instead of the receiver arrangement, I had a Henley's discharger, using respectively for charged electrode the conical point of one of the rods and

* Mascart's *Electricité*, t. ii. § 479.

a spherical ball 0·5 inch in diameter fixed on the point. A circular plate, 7 inches in diameter and fixed on the other rod, formed the uninsulated electrode. The sign of the electricity was changed by merely charging the one or the other paper conductor of the machine. The electrometer had either half-ring capable of being insulated; and I was thereby enabled to take readings in both directions for each kind of spark, and so eliminate any difference due to a bias in the instrument.

With the point for charged electrode the following results were obtained:—

Distance between point and plate.	Difference of potential for positive discharge. (1)	Difference of potential for negative discharge. (2)	Difference of (1) from (2).
½ inch.	76·6	67·1	9·5
1 „	86·3	76·2	10·1
2 „	102·3	95·2	7·1

The behaviour of the index showed that the discharge was not single, but a rapid succession of small discharges; for it first attained a temporary maximum deflection, and then a steady deflection slightly less than the maximum. The latter, on account of its being capable of being observed with greater precision, was the one recorded. This agrees with Prof. Röntgen's* experience. It would have been better if a comparison had also been made at a distance where the discharge could take the form of a spark; but as it is, the observations show that in the region of the brush or convective discharge the curve for the point negative is similar to that for the point positive, and slightly lower. Is this difference due to the electrical state of the air in the neighbourhood of the point?

With the ball as charged electrode the following results were obtained:—

Length of spark.	Difference of potential for positive spark. (1)	Difference of potential for negative spark. (2)	Difference of (1) from (2).
¼ inch.	118·8	129·7	−10·9
½ „	179·6	201·7	−22·1
¾ „	219·2	227·3	− 8·1
1 „	234·6	234·3	·3

In the case of the first two distances the discharge took place in the form of a single loud white spark, the index gave only

* Phil. Mag. Dec. 1878, p. 141.

one reading, and fell back after the passage of the spark almost to its ultimate position. At the third distance, when the charge was negative, hissing sparks, giving only very small discharges (as indicated by the behaviour of the index), preceded the loud spark, which gave complete discharge; but when the charge was changed to positive, no hissing discharges were observed preceding the loud discharge. At the fourth distance hissing discharges preceded the loud discharge in both cases; but they were much more numerous in the case of the negative than of the positive charge.

The readings for the positive discharge and for the negative discharge, when plotted, indicate each a curve similar to that of diag. 6, but with these two differences—first, that in the region of the single spark the negative curve is higher than the positive, and, secondly, that it passes into the brushy state at a less distance and then becomes lower. The readings appear to indicate that the brushy sparks begin at the same difference of potential for the two. Thus the preeminence of the positive spark is due to the fact that it requires a less difference of potential.

This result agrees in one respect with, but differs in another from, that obtained by Wiedemann and Rühlmann * under somewhat similar circumstances. They used a ball 2·65 millims. in diameter, and instead of the plates a larger ball of 13·8 millims. in diameter, which was not insulated like the plate, but connected with the other conductor of the Holtz machine. The pressure was also different, the greatest at which the observations were taken being 55 millims. Their result agrees in this, that the curve for the small ball negative lies above that for the small ball positive for distances between 3 and 12 millims.; and differs in this, that their curve for the small ball positive is convex towards the axis of distances.

Measurement of the Dielectric Strengths of different Substances.

Prof. Chrystal † has pointed out the two modes in which the dielectric strength of a medium may be measured :—1st, by the value of the electrostatic force when the electricity is on the point of passing from one molecule to the next; and 2ndly, by the maximum value of that electric tension which, according to the theory of Faraday and Clerk Maxwell, exists in the dielectric when under induction. He uses the phrase dielectric strength to denote the latter; as it is convenient to have a name for the other physical quantity, it may be called specific resistance. In the following table I have embodied

* Pogg. *Ann.* cxlv. p. 372.

† *Encyc. Brit.* "Electricity," p. 60.

Dielectric.	Relative specific resistance. Comparison at ·5 centim.	Square of specific resistance.	Dielectric strength, $p'=\frac{KV^2}{8\pi\times(\cdot5)^2}$.	Values given by other experimenters.				
				Faraday.	Wiedemann and Rühlmann.	De La Rue and Müller.	Röntgen.	Masson.
Air	1·000	1·000	251·4	1·00	1·00	1·000		
Carbonic acid	·951	·905	227·4	·92	·80	1·065	3287	
Oxygen	·930	·865	217·5	·73	·77	1·000	2402	
Hydrogen	·634	·402	101·1	·53	·65	·547	1296	
Coal-gas	·935	·874	219·8	·71				
Nitrogen	......	......	....	·88	1·09	·746		
Olefiant gas	......	......	....	1·08				
Muriatic-acid gas	......	......	....	1·59				
Carbonic oxide	......	......	....	......	......	......	2634	
Marsh-gas	......	......	....	......	......	......	2777	
Nitrous oxide	......	......	....	......	......	......	3188	
Paraffin oil	3·7	13·7	3442×K					
Oil of turpentine	4·0	16·0	8851	......	......	......	......	16·0
Paraffin (liquid)	2·4	5·76	2896					
„ (solid)	5·0	25·0	12575					
Olive oil	3·5	12·3	3080×K					
Ether	......	......	....	......	......	......	......	7·8
Spirit of wine of commerce	......	......	....	......	......	......	......	8·0
Water	......	......	....	......	......	......	......	11·6
Absolute alcohol	......	......	....	......	......	......	......	12·0

the results of my own measurement, and have also entered values given or deduced from data given by other experimenters.

Column 2 gives the relative difference of potential (or of electrostatic force) required, compared with that for air, to pass a ·5-centim. spark between the parallel disks, the substance being at the atmospheric pressure. It is necessary to make the comparison at a standard distance and pressure; for, as has been shown, the curves for the different dielectrics, when the distance or pressure is varied, are not similar to one another. The gases were compared directly for the length of spark mentioned; and the ratio was taken from the mean of eight or so readings. As the liquids could not be compared directly for so long a spark, the comparison was made at ·4 centim. and the readings reduced to ·5 centim. by means of the appropriate laws. The paraffin examined is of low melting-point. We liquefied it and poured it into the vessel containing the disks, took several ·3-centim. sparks through it in the liquid condition, and, after allowing it to solidify for twenty-four hours, took one spark through it to determine its specific resistance in the solid state. The latter is double the former.

The numbers in column 3 are the squares of those in column 2; they are proportional to the dielectric strengths, provided K, the specific inductive capacity, is constant. In calculating the values of the dielectric strength for oil of turpentine and paraffin, I have taken K, = 2·2 and 2 respectively, from the table in Gordon's 'Electricity'*.

I have calculated the values entered under Faraday from his equivalent mean intervals†, taking those for the positive discharge. It will be observed that the numbers are uniformly less than those of column 2, and that they agree better with those of column 3. This is because the electrodes used were two balls, in which case the difference of potential is proportional to the square root of the distance.

Those entered under Wiedemann and Rühlmann I have calculated from the formulæ‡ which they give for the discharge through different gases at different pressures when the electrodes are two equal balls, the negative one being uninsulated. The comparison is made at 80 millims. pressure. The order is the same as in column 3 and column 5. Those entered under De La Rue and Müller§ and Röntgen ‖ are the values given by these physicists. It will be observed that

* Vol. ii. p. 134. † 'Experimental Researches,' vol. i. § 1388.

‡ Pogg. *Ann.* cxlv. p. 364.

§ Phil. Trans. vol. clxix. p. 95. ‖ Phil. Mag. Dec. 1878, p. 446.

they place carbonic acid differently. This may be due to the difference in the nature of the discharge employed, which may have decomposed the substance in the one case and not in the other.

Masson's* values were obtained by means of the method of Faraday. It is interesting to note that the value he gives for oil of turpentine is, as it ought to be, equal to the square of the specific resistance which I have given.

XLVII. *On the Thermic and Optical Behaviour of Gases under the Influence of the Electric Discharge.* By E. Wiedemann.

[Concluded from p. 380.]

5. *Connexion between Spectral Phenomena, Elevation of Temperature, and Quantity of Electricity.*

I HAVE attempted to solve the problem of the connexion between the quantity of electricity passing through a gas of given pressure, the quantity of energy produced, and the corresponding changes in the spectral phenomena only in one particular case, which, however, seemed to me to be of especial interest. I have determined *what amount of energy is necessary to change the band spectrum of hydrogen into the line spectrum.*

The observations were made in the following way:—A calorimeter was passed over the horizontal capillary tube of a discharge-tube, and the spectrum was observed, as near as possible to the place where it fitted on the tube, by means of a spectroscope with a horizontal slit. The band spectrum was then changed into the line spectrum by interposing air-sparks. If the spark-length is gradually increased, beginning with none, the band spectrum is at first very bright; but soon the three hydrogen-lines make their appearance beside it, then the ground becomes fainter and the hydrogen-lines brighter, until at last the band spectrum disappears almost suddenly. The change is so sudden that it may often be recognized by the eye without the spectroscope. This order of phenomena has already been observed by Wüllner and others. When the spark-length necessary had been determined, the rise in temperature of the calorimeter t and the number of discharges were determined. From this the rise of temperature for one discharge at the given pressure was calculated, and then, by dividing by the pressure, what the rise of temperature would have been if the pressure had been only 1 millim.

If a denote the rise in temperature of the calorimeter in a minute caused by a current of deflection 100, W the water-equivalent, z the number of discharges in a second with the

* *Ann. de Chim. et de Phys.* 3rd ser. t. xxx. p. 25.

same current, and p the pressure, then the value

$$A = \frac{a}{60pz} W$$

expresses what quantity of heat must be evolved in order to change the band spectrum into a line spectrum in a gas of unit-pressure, in which of course the assumption is made that for a given volume the above quantity of heat is proportional to the pressure. The measurements were made with two different tubes, of radii 0·304 and 0·183 millim., and are given in the following tables *without exception*, and in the order in which they were made. The length of the portion of tube in the calorimeter was 42 millims.; the water-equivalents of the two calorimeters were 11·84 and 12·08. Some of the numbers were certainly too small, since at the moment the striking-power of the machine did not rise sufficiently to cause a complete disappearance of the band spectrum. These are marked with an asterisk, and are left out in determining the mean. Two different series of observations were made with the first tube, of which one includes the first three observations and the other the last four.

I.

p.	a.	z.	A.
5·8	0·50	166	0·0000086*
2·2	0·28	184	0·0000116
0·9	0·19	181	0·0000191
15·3	0·745	100	0·0000081*
5·4	0·50	96	0·0000158
2·1	0·28	160	0·0000139
0·9	0·21	233	0·0000167
		Mean	0·0000155

II.

p.	a.	z.	A.
9·4	0·52	250	0·00000369*
3·6	0·26	200	0·00000615
1·7	0·20	270	0·00000726
6·2	0·35	160	0·00000587
14·1	0·60	133	0·00000533
		Mean	0·00000565

Two determinations at pressures 2·2 and 0·7 millims. gave for the tube I, whilst bands were still plainly to be seen, A=0·0000052 and 0·0000057.

The numbers in the last columns of I and II agree as well as the complicated conditions of experiment allow. They show that "the quantity of energy absorbed from the discharge to change the band spectrum into the line spectrum for a given quantity of hydrogen is independent of the pressure." The ratio of the quantities of heat necessary per unit of area in order to convert band spectrum into line spectrum in I and II is, since the lengths of tubes giving off heat are equal,

$$\frac{(0{\cdot}183)^2\times 0{\cdot}0000155\times 11{\cdot}84}{(0{\cdot}304)^2\times 0{\cdot}00000565\times 12{\cdot}08}=\frac{617\times 10^{-8}}{631\times 10^{-8}}=0{\cdot}979;$$

a number as nearly equal to unity as can be expected in experiments of such unusual difficulty.

We may hence conclude that the quantity of heat to be given up by the discharge to the unit weight of gas in order to change the band spectrum into the line spectrum is independent of the pressure and of the section of the tube. We may also easily calculate (1) *how great the quantity of heat is in absolute measure necessary for* 1 *gr. hydrogen to effect this change;* (2) *how great a quantity of electricity is necessary at any given pressure.*

The water-equivalent of the calorimeter I is 11·84, the rise of temperature for each discharge 0·0000155, the length of the portion of tube in the calorimeter 4·2 centims.; so that the quantity of heat given per unit length in each discharge is

$$\frac{11{\cdot}84\times 0{\cdot}0000155}{4{\cdot}2}\text{ calories.}$$

The volume of the gas per unit length is, since the section of the tube is 0·0029 square centim., 0·0029 cubic centim., and its weight at 1 millim. pressure $\frac{1}{760}\times\frac{1\times 0{\cdot}0029\times 0{\cdot}069}{773{\cdot}38}$ gr.

The quantity of heat necessary for 1 gr. hydrogen is therefore

$$\frac{11{\cdot}84\times 0{\cdot}0000155\times 760\times 773{\cdot}3}{4{\cdot}2\times 0{\cdot}0029\times 0{\cdot}069}=128300\text{ calories.}$$

Let us further determine the quantity of electricity per unit area of 1 square millim. to convert the band spectrum into the line spectrum.

If we choose the case when the pressure was 2·2 millims. in the first tube, the number of discharges for a deflection of 100 (when the band spectrum changes to the line spectrum) was about 200. A deflection of 0·5 corresponds therefore to one discharge in which the quantity of electricity passes across a section of 0·29 square millim.; so that the quantity of electricity per square millimetre corresponds to a deflection 1·73,

$$=\frac{0{\cdot}000336\times 1{\cdot}73}{100}=0{\cdot}00000581\text{ Daniell-Siemens.}$$

Experiments on the Discharge in very high Vacua.

1. Reitlinger and Urbanitsky*, Spottiswoode†, and others have found that on approaching the finger to, or on touching a *highly exhausted* Geissler's tube, the discharge is deflected. I have further shown‡ that this phenomenon occurs especially with the positive discharge, and that sometimes the inner and sometimes the outer surface of the tube phosphoresces with green light.

By new experiments I have established that the point of the tube put into contact with the ground behaves in every respect like a negative electrode; for

(*a*) a faint reddish light shows itself on the inner face of the tube-wall, the spectrum of which is exactly similar to that of the red glow which forms about the negative electrode;

(*b*) the discharge given off from the wall by a positive or insulated electrode throws images on the opposite wall, which are displaced on the approach of a magnet.

(*c*) if the apparatus (fig. 10) be employed, and *a* be connected with the positive pole of the machine, *b* with the negative pole, and if the wall be touched at *d*, then at *f* there is formed a magnified shadow of *b*, in the same way as one negative electrode throws a shadow of another. The properties and forms of these shadows have been thoroughly studied by Goldstein§.

The discharge sometimes loses the property of sensitiveness in a very remarkable manner, as Spottiswoode has observed. An experiment which I have made shows this very plainly. The tube used was 5 metres long and 5 millims. wide. It was covered with a spiral strip of tinfoil for a distance of about half a metre. If the discharge was rendered sensitive by interpolating air-sparks, and the tinfoil strip connected with the ground, there was produced on the glass wall opposite to it a similar spiral of phosphorescent light, which became continually feebler from the positive electrode towards the negative, and at last disappeared. Also at points more than two metres from the point of contact there was hardly any green light produced by approach of the finger. It was as if the discharge had changed its form of motion, and lost the power of producing phosphorescence.

It is to be noticed that the green light of the glass does not appear most brilliantly in the parts of the tube traversed

* Reitlinger and Urbanitzky, *Beibl.* i. p. 416 (1877).

† Spottiswoode, *Beibl.* iii. p. 643 (1879).

‡ E. Wiedemann, Wied. *Ann.* ix. p. 157 (1880).

§ Goldstein, *Berl. Ber.* p. 284 (1876), and *Eine neue Form electrischer Abstossung* (Berlin, T. Springer, 1880).

by the current, but at points where blind branch tubes are placed, as, for example, in the pieces joined on for the purpose of securing the joints (Pl. IX. fig. 6).

2. It was further examined whether the negative discharges which excite the green light, the so-called kathode-rays, do really propagate themselves only in straight lines. For this purpose the apparatus fig. 11 was employed. If *a* was negative and *b* positive, the discharge from *a* struck first upon the glass wall at *s*, causing it to shine brightly, and then upon the bend at *m* (when the angle at *m* was about 30°), where bright green light also appeared. On the other hand, the tube was only feebly illuminated from *s* to *m* and *m* to *l*; whilst on the surface *d*, exactly opposite the mouth of the tube *s m l*, there appeared a bright green spot, in the midst of which there was a sharp shadow of the electrode *e*.

That this spot of light is not produced by light which has undergone several reflections in the interior of the tube *s m l* may be seen in two ways: first, there was no violet spot when the positive discharge traversed the tube without the production of green light; and, further, the green spot alters its position under the influence of a magnet.

If the angle at *m* is chosen greater than 45°, and, moreover, the tube *m l* is taken tolerably long, the spot is at first faint, but appears with greater brilliancy as soon as the part of the tube surrounding *a* is touched directly with the finger. At the same time the discharge issuing from *a* within *a s* is hardly affected by the magnet. Touching a point at some distance from *a* is without influence.

Whether we have to do here with an actual bending of the kathode-rays, or whether the result is due to secondary action, must be further investigated. It is possible that the wall at *m* acts as a negative electrode, and so causes the deviations of the rays. This supposition is supported by the fact that, if a glass rod or a narrow metallic plate be brought into the path of the bundle of rays diverging from a plane surface and near to the kathode, the rays diverge from it in a high degree, as if it were itself a negative electrode. The divergence is increased by interpolating air-sparks in the circuit.

3. Further, to determine whether the negative vibrations which excite the green light could traverse the ordinary positive discharge, the following experiment was made:—

a (fig. 12) is an electrode in the form of a disk, *b* is a point. When *a* is negative and *b* positive, a bright green spot appears at *m*, corresponding to the opening of the tube at *c*; at the same time the tube at *c* shines with violet light of the same colour as a tube through which no kathode-rays pass.

An experiment to determine whether there was any difference in time between the appearance of the violet light and of the green gave no reliable result—partly, no doubt, because of the feebleness of the light at *c*.

4. I have, further, made experiments to determine whether the kathode-rays propagate themselves with a measurable velocity; and employed for this purpose a discharge-tube 1 metre long, provided with a disk electrode at one end, which at a sufficient exhaustion sent the kathode-rays through the entire tube. When the tube was viewed in a revolving mirror, the phosphorescence appeared at the same instant at both ends of the tube.

5. In order to examine the influence of the shape of the electrodes on the number of discharges, and the potential necessary to produce them, the following experiments were made.

The following pieces of apparatus were connected together by fusion onto a forked tube:—1st, the apparatus fig. 11 from *a* to *m*; secondly, the apparatus fig. 10; thirdly, a discharge-tube similar to fig. 11, except that at *b* there was a disk parallel to *a*. The whole was exhausted, so that when the disks were negative the whole tube shone brightly with green light. In the revolving mirror the following results were obtained:—

(*a*) With the apparatus fig. 10, no difference whether the positive or the negative electrode was put to earth; the green band of light appeared almost entirely continuous.

(*b*) With the apparatus with the two disks the same is the case.

(*c*) With the apparatus fig. 11, when the disk is negative no discontinuity is observed in the revolving mirror; but if it is positive, then the appearances represented in fig. 13 are observed. They consist of single, sharp, very bright images of the tube, e. g. *b* and *d*; at the side of which is seen a much feebler preceding discharge, *a a*, *c c*, sharply bounded at its commencement and of nearly equal intensity throughout. If the velocity of rotation of the electrical machine be increased, the appearance is not altered. If the outside of the tube be connected with the earth, the broad strips are resolved into separate images. The appearance is probably to be explained by the charge taken by the tube. At very small pressures, therefore, not only the number of the discharges, but the whole mechanism depends to a great extent on the form of the electrodes.

6. In order to measure the influence of the kathode-rays radiating from the negative electrode on the intensity of the current, a mica disk provided with a hinge was so placed between two disk electrodes opposed to each other at a distance

of about 40 centims., that without altering the connexions it could be brought between the electrodes or removed. In the former position it intercepted the kathode-rays issuing from the negative electrode, whilst in the latter they had a free path. The intensity of the current was the same in both cases.

7. Reitlinger* has shown that when stratifications form about the positive electrode in a tube traversed by a current, their number is considerably increased by the approach of a magnet from the negative pole. The increase is caused by the advance of those nearest the negative electrode, and the appearance of new ones at the positive pole. I have observed that the mean distance of the layers is not much altered in the process. When, by carrying the exhaustion far enough, all the stratifications have been made to disappear, I have succeeded in producing new ones by approaching a magnet to the electrode. The phenomenon is especially beautiful when the positive electrode is a point; the stratifications seem then to float up round it.

The phenomena are seen most distinctly when the quantities of electricity passing in the discharge correspond accurately to the potential necessary to cause discharge through the tube, and are not increased by interposition of air-sparks in the circuit.

Action of different Sources of Electricity.

The conditions of discharge are certainly the most simple in *friction-* or *electrophorus-machines*. The electricity is produced uniformly, it flows into the electrodes, after a certain time reaches the potential necessary to discharge, and discharges itself, and the process begins anew. So little electricity is collected upon the disks, conducting-wires, &c., or the flow of electricity is so rapid, that the discharge may be considered almost instantaneous. It is only in quite exceptional cases at very low pressures that we see sometimes how a discharge is made up of a series of partial discharges.

But for our investigations we must have such instantaneous discharges; for only with such can we assume that the discharge gives up its energy to the gas so rapidly that no perceptible transference of energy from the gas to its envelope takes place during the process.

The quantities of electricity produced by Töpler's machine are abundantly sufficient for all thermic measurements, while, on the other hand, they are not so great that the images of the separate discharges cannot be distinguished in the revolving

* Reitlinger, *Wien. Anz.* p. 795 (1876).

mirror. It is, further, of importance that the quantity of electricity produced can be varied simply by a change in the velocity of revolution of the machine and without alteration of the connexions. The introduction of resistances (which would cause complications in the processes of discharge) is therefore rendered unnecessary.

By the introduction of resistances into the circuit the mode of discharge is in general not altered, but the quantity of electricity which passes suddenly in each discharge is increased. Conditions are much more complicated when the induction-coil is employed*.

At each breaking and closing of the primary current we have, besides the principal discharge, a whole series of partial discharges of decreasing intensity. That we have not only a single discharge traversing the tube is seen by the use of the rotating mirror. But each of the partial discharges lasts a certain length of time, during which the electricity flows gradually from the coil to the electrodes ; it will therefore again either be itself composed of a series of partial discharges, or correspond to a more or less continuous discharge of electricity. The images of the discharges do not consist of separate sharp lines, as with the electrophorus-machines, but of more or less broad bands in which maxima and minima occur.

The total heating in the tube is, as shown above, always dependent upon the total quantity of electricity transmitted, without reference to the quantity in each discharge; so that the result obtained by Naccari and Bellati† is intelligible without further explanation.

Hence it results that at different pressures of the gas in the discharge-tube the duration of the induced currents, and the quantity of electricity furnished by the inductorium in each discharge, is essentially different, since the potentials at the ends of the coil must rise to different magnitudes before the flow of electricity takes place. If the pressure of the gas be very high, then no doubt only the principal discharge passes through.

From all these considerations, it follows that the induction-coil is not suitable for the quantitative study of the discharge or of spectral phenomena ; for each of the partial discharges, being of a different strength, will produce a different spectral phenomenon, so that one may produce a line spectrum and another a band spectrum.

Exactly similar remarks hold good also for the discharges of large batteries of Leyden jars, only that here another circum-

* Compare G. Wiedemann, Pogg. *Ann.* clviii. p. 286 (1876).

† Naccari and Bellati, *Beibl.* ii. p. 720 (1878).

stance must be taken into account. A part of the very large quantity of electricity passing in each discharge charges the surface of the tubes and discharges itself only very gradually (in ten or more minutes), as is clearly shown by the afterglow of the tubes. This is particularly striking when very long tube conductions (30 metres and more in length) are employed.

In considering *galvanic batteries*, such as Hittorf employed in his beautiful investigations, we must first examine whether, with the large quantities of electricity produced, the gas returns after each discharge to its original condition.

In order not to complicate the inquiry unnecessarily, let us imagine the tube replaced by an indefinite space bounded by two parallel planes. Let the whole of the gas be heated by the instantaneous discharge to the same initial temperature A. Let the distance of the walls be c, their temperature constant and equal to zero, the temperature of the separate points between the plates u, their distances from the one plate x, and the time t. Then the flow of heat, if a^2 is a constant, is determined by the partial differential equation

$$\frac{du}{dt}=a^2\frac{d^2u}{dx^2},$$

where

$$u=f(x)=\mathrm{A} \text{ when } t=0,$$

$$u=0 \qquad \text{when } x=0 \text{ and } x=c,$$

The solution gives, if r be a whole number,

$$u=4\mathrm{A}\sum_{r=0}^{r=\infty}\frac{e^{-a^2\left(\frac{(2r+1)\pi}{c}\right)^2 t}}{(2r+1)\pi}\sin\frac{2r+1}{c}\pi x.$$

Let us consider only the temperature U in the middle of the plates, then $x=\frac{c}{2}$. If we expand the series and content ourselves with the first term, we have

$$\mathrm{U}=4\mathrm{A}\frac{e^{-\frac{a^2\pi^2}{c^2}t}}{\pi}.$$

For a pressure p in air,

$$a^2=\frac{0{\cdot}25\,.\,760}{p}\,\frac{\text{centim.}}{\text{sec.}},$$

according to Stefan*.

If we take the pressure at $\frac{1}{100}$ atmosphere, which corre-

* Stefan, *Wien. Ber.* lxv. (2) p. 45 (1872). See also Kundt and Warburg, Pogg. *Ann.* clvi. p. 194 (1875).

sponds to ordinary conditions in our discharge-tubes, then $a^2=25$; and if, further, we bring together the constants in U, we have, nearly,

$$U=1\cdot 3\,Ae^{-245\frac{t}{c^2}}.$$

If we take two tubes, of which the one is about 1 centim., the other about 1 millim. wide, then for these, if A_1 and A_2 denote the initial temperatures,

$$U'=1\cdot 3\,A_1e^{-245\,t}, \text{ and } U''=1\cdot 3A_2e^{-245\times 100\,t}.$$

A numerical calculation, neglecting the factor 1·3, shows that in the middle of a tube of 1 centim. width, after $\frac{1}{100}$ second the rise of temperature will have sunk about $\frac{1}{11}$, after $\frac{1}{1000}$ second $\frac{1}{1\cdot 3}$, and that, on the other hand, in a tube of 1 millim. diameter the same changes will have taken place after the $\frac{1}{10000}$ and the $\frac{1}{100000}$ second. If we assume that in the narrow tube the gas has attained a temperature of 1000°, a case easily realized, then (again neglecting 1·3) after the lapse of the times t the temperatures θ (which, it is true, give only a measure of the order of magnitude of those actually present) would be

$$\begin{array}{lllll} t=10^{-6} & 10^{-5} & 10^{-4} & 10^{-3} \\ \theta=980 & 815 & 133 & 1\cdot 4\times 10^{-6}. \end{array}$$

If, therefore, there are 1000 discharges in the second, the gas is after each almost in its original condition; if there are 100,000, this is not the case.

These considerations hold good for the case of a capillary tube, where, in fact, a considerable rise in temperature accompanies the luminosity. As yet no afterglow has been recognized with certainty following the separate discharges. Also in wider tubes experiment shows us that, contrary to theory, no afterglow occurs—a fact to which we will return later.

The above discussion can of course only furnish a measure of the order of the magnitudes in question. An exact discussion is as yet not possible, the constants not being certainly determined. Similar methods would hold good if we desired further to take into consideration the radiation of the gas.

The experiments of my father, of Röntgen, myself, and others have shown that with a uniform supply of electricity a perfectly definite potential is necessary to discharge—that, further, the electricity passes in separate discharges which are almost instantaneous, and which do not appear lengthened in the rotating mirror. With a galvanic battery the electricity used in each discharge is rapidly supplied again; and it is a

question whether we have to do here with discontinuous or continuous discharge.

The current from a Hittorf's cell, when newly put together, will liberate a maximum of 90·2 milligrammes of silver in a minute; so that this would in general represent its maximum strength of current*.

The current of my Töpler's machine corresponded to 0·0006 Siemens-Daniell for a deflection of the galvanometer of 100 millims., and would therefore separate about $77 \times 0{\cdot}0004$ milligramme of silver. It is therefore $\frac{90{\cdot}2 \times 10000}{77 \times 4}$ or about 3000 times as weak as that employed by Hittorf. But with this current I obtained, with electrodes widely separated by capillary tubes, about 2000 discharges in a second at a pressure of about 5 millims. If we assume that Hittorf's battery would furnish the same quantity of electricity as given above when used with the discharge-tube, we should have $3000 \times 2000 = 6{,}000{,}000$ discharges in the second. If the part of the tube seen in the revolving mirror have a breadth of 1 millim. and the mirror make 100 revolutions per second, then, when the observer and the tube are each at a distance of half a metre from the mirror, the image would appear to move $1000 \times 100 \times 2 \times 3{\cdot}14$, or 600,000 millims. in a second. Accordingly, six separate discharges would appear to fall upon each millimetre of the image. But as the impression of the light on the retina lasts more than one seventh of a second, we see the images corresponding to 14 revolutions of the mirror at the same time; so that the images of 126 separate discharges are seen together in each millimetre.

In the tubes employed by Hittorf the electrodes were much nearer together, and the pressures were lower; so that the number of discharges would be further considerably increased.

With so large a number of discharges the heating and luminosity of the gas does not sink again to nothing between each two discharges; so that the separate images appear lengthened out in the mirror. Hence it is here quite impossible to draw conclusions about continuity or discontinuity of discharges. Moreover the criterion given by Hittorf to decide the question of continuity—the want of sensitiveness of the discharge on approach of the finger—is not always applicable, as experiments with the Töpler's machine have shown that the discontinuous discharges are almost altogether non-sensitive; but they can be made more or less sensitive by the interpolation of air-sparks of definite length. Most probably, in most

* Hittorf, Wied. *Ann.* vii. p. 557 (1880).

cases, the discharge of the battery, according to the conditions existing in the battery itself, is either altogether discontinuous, or takes place in such a way that stronger discharges cause the chief discharge, upon which follows a continuous discharge gradually becoming weaker, and so on.

But even if the discharges are completely continuous, I do not believe that they can be employed in spectroscopic investigations, since the final temperature of the gas depends on so many circumstances which cannot be exactly determined—for example, upon the radiation-coefficient, which varies with the temperature.

Theoretical Considerations.

If we endeavour, from the above facts and others yet to be mentioned, to represent to ourselves the mode in which the electric discharge takes place through a gas, we see, in the first place, that the theory proposed by G. Wiedemann and R. Rühlmann is not supported by these facts. According to this theory, the discharge is carried on by molecules charged with electricity which are driven off from the electrode, and which, upon collision with other molecules, yield up their electricity to them. The same theory has been adopted, in a somewhat different form, by Crookes to explain the phenomena described by him, but most of which, though no doubt without his knowledge, had been long ago made known by Hittorf, and then by Goldstein and others. According to this theory, the molecules must possess velocities corresponding to the velocity of propagation of electricity in gases. But we know, from Wheatstone's* experiments on the discharge in gases, that this is certainly greater than 200,000 metres, or 30 (German) geographical (=124 British statute) miles. But it is quite certain that the molecules do not possess so great a velocity of translation measured in the direction of the current. Dr. von Zahn has observed the lines of the spectrum given by a Geissler's tube, first when the axis of the tube and the axis of the collimator of his spectroscope were parallel, and then when they were at right angles, and observed no displacement of the lines, although the dispersion was so great that a displacement of $\frac{1}{40}$ of the distance between the D-lines could have been observed. A displacement of this magnitude would, in accordance with Doppler's principle, have corresponded to a velocity of 4 geographical miles; and a velocity of 30 geographical miles would have produced a displacement nearly equal to the entire interval between the

* Wheatstone, Pogg. *Ann.* xxxiv. p. 464, 1835.

two D-lines. So great an increase of the normal velocity of the molecules is therefore quite improbable *.

According to my view, we may imagine to ourselves the phenomena of the discharge somewhat as follows:—The electricity produced by the machine, which we may imagine as free æther, is accumulated on the surface of the electrodes partly as free electricity, and there is prevented from passing into the surrounding gas by the mutual action between it and the molecules of the metal; and a transference can only take place when its density has reached a sufficient magnitude. At the same time the electricity produces a dielectric polarization in the surrounding medium in such a manner that the æther envelopes of the separate gas-molecules become deformed, and, in consequence of the rotation of the molecules on their axes, maintain a definite position. If a discharge takes place, the sudden change of the dielectric polarization thereby produced propagates itself from the electrode through the æther envelopes of the gas-molecules, and thereby puts them into vibration. At the same time a transference of free electricity from the electrode may no doubt take place from molecule to molecule.

As a ray of light produces in the æther envelopes of the molecules of phosphorescent and fluorescent bodies oscillatory movements whose *vis viva* is considerably greater than corresponds to the temperature, so also the case is here ; as there the motions of the æther which produce the luminosity gradually transfer themselves to the molecules and produce heat-motions, so also, in an exactly similar manner, with the electric discharge a secondary elevation of the total temperature results.

If in consequence of this transference two molecules of the gas have a greater motion of oscillation than corresponds to their temperature, in accordance with the normal relationships between motion of translation and motion of oscillation (rotation), then upon their collision a portion of the internal motion resolves itself into motion of translation, until at last the normal condition is restored †.

* These considerations, which I had already (Wied. *Ann.* ix. p. 160) brought forward to prove that there is no propagation of molecules in the direction of the current, have been again adduced by Goldstein in a paper (Phil. Mag. September, 1880) on the phenomena which take place at the negative pole, which only came to my knowledge since the above was in type.

† In the paper cited above I have compared the luminosity of the gas under the influence of the discharge with the light of a fluorescent substance. Hittorf, in discussing it, has expressed the opinion that one should speak of phosphorescence. The two terms, however, are only dif-

That there is really such an excess of internal motion in gases rendered luminous by electricity is shown by their low temperature. The change of motion of oscillation into motion of translation takes place with very great rapidity. I have shown on p. 416 that, according to the laws of conduction of heat, in a tube 1 centim. wide, even after $\frac{1}{1000}$ second, a perceptible fraction of the original energy must exist in the centre of the tube, yet nevertheless it is seen in the rotating mirror to be discontinuously illuminated, just as a narrow tube would be; so that we must conclude that the transference of the internal motions produced by the current into heat is much more rapid than the conduction of heat itself in the gas.

The vibrations produced by the electric discharge may become so energetic that the molecules themselves fall to pieces and are resolved into their constituent atoms, just as we observe decomposition when we receive the chemically-active rays on silver chloride, or an increase of affinity when they fall upon chlorine.

If the oscillatory motions produce a decomposition of the molecules, the energy necessary for the decomposition is brought to the molecules from the source of electricity, and is again given up to the calorimeter when they reunite. Whether the whole quantity of heat produced in this case results from the latter process cannot be decided off hand.

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ferent names for the same phenomenon. Fluorescence and phosphorescence differ only in degree. Fluorescent light lasting really for only an instant could never be observed. The time during which the luminosity lasts is determined by the rapidity with which the internal motions of the molecules equalize themselves, and consequently by the mutual influence of the molecules in their internal motions. With solid bodies this is certainly relatively small: the molecules vibrate about their positions of equilibrium; and the elongations are so small that the actions of neighbouring molecules on the different parts of the same molecule are very little different. Only thus can we explain and understand the appearance of sharp absorption-bands in the case of solid bodies, since light emitted by solid bodies can still interfere when there is a great difference of path (compare Wied. *Ann.* v. p. 500, 1878). The same holds good for fluids: here also, besides the motion of translation of the molecules, we have vibrations of the whole molecules; for otherwise the law of change of friction with temperature would be the same for gases and for fluids, whereas it increases with the temperature in the former case and decreases in the latter. With liquids also the time during which a molecule vibrates uninfluenced is therefore relatively large, and the transference of energy follows relatively slowly. This will take place with gases most easily and rapidly, where the mean path-length alone is of account; and therefore the name fluorescence is first applicable here. But here also the fluorescent light will last at least as long as the mean interval between two collisions.

It is possible that a first fraction of the electricity transmitted is used up in producing an increase of the mean temperature resulting from a transformation of oscillatory motion, that by this an unstable condition of the molecule is brought about, and the remainder of the electricity completes the decomposition.

There is a remarkable parallelism between the production of heat caused by a ray of light in a feebly absorbing medium and that which is caused by the electric discharge.

(1) If a conically enlarging beam of light propagate itself in a feebly absorbing medium, the quantity of heat developed is nearly the same in every section of the beam; in the same way the heat produced by an electric discharge is the same in each section.

(2) If we increase the intensity of the ray of light, but allow it to pass for a proportionately smaller time, an equal quantity is absorbed in the two cases; the same holds good for the discharge when we increase its strength, but decrease its frequency.

(3) If we increase the optical absorption by, for example, increasing the number of absorbing particles in the absorbing solution, the quantity of heat produced is increased in proportion; so also the electric evolution of heat in a gas becomes greater for the same section as the pressure increases.

It seems therefore very probable that the absorption of energy in both cases follows in the same way, that therefore the discharge consists in a propagation of vibrations which give up a portion of their energy to the gas-molecules. But then we must make the assumption that the amplitude caused by a quantity of electricity e is proportional not to e, but to $\sqrt{e}$.

But even under this assumption the phenomena of the electric discharge cannot be explained so well as the optical phenomena, since so many disturbing causes occur.

Amongst them must be mentioned, first of all, the statical charge of the glass walls, which largely affects the transmitted discharge, and produces by itself a definite polarization of the gas-molecules, sometimes increasing and sometimes diminishing the energy given up by the discharge. These exterior charges produce most effect with wide tubes, in which the motion produced by the discharge itself is much less rapid than in narrow tubes. Here should be mentioned the interesting fact discovered by Hittorf, that a gas which is traversed in one direction by a discharge conducts the current in a direction at right angles to that much better than when this is not the case.

The great differences in the behaviour of positive and negative electricity may perhaps be explained by assuming that the propagation of negative electricity alone depends upon the communication of electric polarization, whilst that of positive electricity is associated with a transference of free æther from molecule to molecule, as indeed Von Ettingshausen * has recently concluded from the experiments of Hall †.

We should thus have explained at once the difference in potential necessary for the commencement of discharge from the negative and positive electrodes. At the negative electrode the resolution of the dielectric polarization alone is necessary, whilst at the positive the attraction of the material molecules for the æther has also to be overcome.

The propagation of the discharge issuing from the negative electrodes obeys nearly the laws of light. From each point of the electrode there apparently issues a feebly diverging bundle of kathode-rays, which behaves in its further course almost exactly like a beam of light, casting shadows, producing phosphorescent light, and so on. But we must not look upon the electrode as itself the source of light, but as a surface parallel to a wave-surface. For in the source of light the displacements of the æther envelopes of the separate molecules are still altogether without law, and take place indifferently in all directions. The wave itself, all of whose parts vibrate isochronously, is first formed at a definite distance from the electrode; whilst at the electrode the dielectric polarization possesses a perfectly definite position, and is simultaneously resolved at all points. The superficial molecules of the electrode itself do not form a wave; for the displacements do not occur in them in the same manner as, for example, when the kathode-rays issuing from the negative electrode produce phosphorescent light when they strike the glass wall in the same way as light-waves. If, for example, a part of the outer wall of an exhausted glass tube be covered with a metallic coating and this be connected with the induction-coil, the inner wall of the tube at this point becomes alternately negative and positive; nevertheless I have never been able to observe the green light on it. Leonh. Weber ‡ has also recently employed such exterior coatings for the study of Hittorf's phenomenon.

The deviation of the positive discharge, by connecting the outer surface of the tube at any point with the earth, would be an effect of the altered potential produced by the altered dis-

* Von Ettinghausen, *Wien. Ber.* lxxxi. March 4, 1880.
† Hall, Phil. Mag. [5] vol. ix. p. 225, 1880.
‡ L. Weber, *Carl. Rep.* xvi. p. 240, 1880.

tribution of the free electricity on the walls of the tube on the free current-electricity. The wall, turned towards a positive discharge, then naturally plays the part of a negative electrode. In what way the discharges issuing from the two electrodes equalize themselves is still an open question.

I have already shown *, and also mentioned above, that the phenomena produced by the kathode-rays cannot be caused by projected molecules. If we were even to assume that the thickness of a layer of such projected particles amounted to a whole centimetre, that it therefore possessed a mass relatively great, yet, in order to produce the observed heating of the tube above the negative electrode, it must possess velocities of 100,000 metres or more.

Gintl † and Puluj ‡ seek to refer the phenomena to a projection of the substance of the electrodes; but since with aluminium electrodes, for example, no decrease of the electrodes is observed, the velocity must be immense. This view is therefore scarcely tenable. Moreover all the other phenomena observed may be explained by the propagation of æther-waves of very great energy; thus, for example, the mutual influence of two rays may be explained by the pressures occurring on the anterior surface of advancing waves §, which in our case may be of considerable magnitude.

Let us turn, lastly, to the results in spectrum-analysis which may be drawn from the above investigation. In a previous investigation ‖ I have endeavoured to show, in continuation of the investigations of Lockyer, Lecoq de Boisbaudran, and others, that the band spectrum corresponds to the individual molecule, the line spectrum to the separate atoms. The same quantity of heat which is necessary to change the band spectrum into the line spectrum gives, therefore, a maximum value for the heat of decomposition of the hydrogen molecules, of which, besides the work expended in the dissociation, another portion is employed in heating the gas.

We find that *about* 128,000 *gramme-calories must be communicated to* 1 *gramme of hydrogen of ordinary temperature in order to separate it into its atoms.* This number is to be regarded as only a rough approximation, since its determination is affected by so many sources of error; it must remain for further investigations to determine this number more exactly.

It corresponds to values such as we frequently have in

* E. Wiedemann, Wied. *Ann.* ix. p. 157, 1880.
† Separate Publication.
‡ Puluj, *Wiener Anzeiger*, p. 76, 1880.
§ Maxwell, 'Electricity and Magnetism,' ii. p. 391.
‖ E. Wiedemann, Wied. *Ann.* v. p. 512, 1878.

thermochemical decompositions. A determination of the quantity of heat necessary for the decomposition of the molecules of the elements must be important for the science of chemistry. If, for example, we have the reaction

$$\mathrm{H\,H + Cl\,Cl = 2\,HCl},$$

the thermic effects are of three kinds:—first, heat is absorbed in separating one atom of hydrogen from the other; a further quantity of heat is absorbed in separating one atom of chlorine from the other; and, lastly, twice as much heat is produced as corresponds to the formation of one molecule of hydrochloric acid.

Until now only the total result has been measured. It is now possible, by spectroscopic methods, to determine the first two portions by themselves, and so to determine the true heat of combination.

If we assume, as is in the main confirmed by the preceding investigations, that the spectra are conditioned by the quantity of electricity given to each molecule and the energy absorbed by it, the known dependence of spectra on the pressure may be explained as follows:—

When the pressure is great in a discharge-tube, the number of gas-molecules is large also. As at the same time the number of discharges for an equal supply of electricity is comparatively small, the energy given up by each separate discharge must be relatively very large. At the same time this may be confined to a few molecules, in consequence of the formation of a small spark. A very great quantity of energy is given to each molecule; it decomposes into its atoms, and we have a line spectrum.

If the pressure falls, the number of discharges increases, the energy given up decreases, and, moreover, all portions of the gas are put into motion: hence the number moved is larger, a smaller quantity of energy is given to each molecule; it is no longer sufficient for the decomposition of molecules into atoms, and the spectrum is a band spectrum.

If now we include air-sparks in the circuit, the quantity of electricity transmitted in each discharge is increased, and consequently the energy given up; hence the band spectrum is converted into the line spectrum. At very low pressures complications occur which cannot be dealt with till we possess a complete theory of the discharge.

If we find in the experiments (p. 407) that the line spectrum appears together with the band spectrum, first weak and then increasing in strength, this is in entire accordance with the conclusions which follow from the kinetic theory of gases.

Of all the molecules present in the gas, there are always some in which the atoms perform such vibrations that a very small impulse is sufficient to separate them. Such impulses are communicated to the atoms by the discharge in abundance; hence the hydrogen-lines appear already in tubes of relatively great width, with a very small supply of electricity to each molecule. So, on the other hand, there are molecules present for the separation of whose atoms comparatively powerful impulses are necessary; the complete disappearance of the band spectrum takes place therefore only very gradually. In exactly the same way we understand why, with the discharges of the induction-coil, we have the line spectrum and the band spectrum present together: it is to be observed that the discharge of an induction-coil always consists of partial discharges of varying strength.

It was shown on p. 372 that, when electrodes of different forms are employed, the heat produced in a capillary tube is the same, and moreover the number of discharges does not alter much. This fact shows that in a capillary tube, if we have to do with discontinuous discharges, we can never have spectra corresponding to quantities of electricity less than a certain minimum value.

The spectra which result from the smallest quantity of electricity transmitted in each discharge, and the smallest quantities of energy given off, are obtained by placing the two electrodes close to each other and exhausting till the spark-discharge ceases and the silent discharge alone takes place. A cylindrical portion of the gas of several millimetres diameter becomes luminous, and is traversed by a large number of discharges. The energy given off may indeed fall so far that scarcely any light at all is to be seen.

The only means which we have of obtaining spectra of small energy of internal motion with the necessary brilliancy consists in the employment of wide tubes. We may get rid of the influence of thickness by employing tubes with axial line of sight; but I have never been able to observe any difference between nitrogen seen in this way and nitrogen seen transversely. Hittorf has found similar negative results with sodium-vapour.

In order to compare the action of the induction-coil with that of the electrophorus-machine in producing spectra, I excited a Stöhrer's induction-coil of medium size by the current from four Bunsen cells, and employed as current-break an automatic tuning-fork giving 100 complete vibrations; the one pole of the coil was connected directly with the earth, and

the other connected by a discharge-tube and galvanometer with the earth.

The deflection amounted to 145 millims., with a pressure of about 10 millims. in the discharge-tube; so that a deflection of about 1·5 millim. would correspond to a discharge. The quantity of electricity given by each discharge would therefore be sufficient to convert the band spectrum into the line spectrum in a tube of 1 to 2 millims. width (p. 409). But if we have even a very powerful excitation of the coil, and replace the metallic contacts by a mercury break, we shall hardly succeed in bringing to bear on unit surface in a tube 10 times as wide, and consequently of 100 times the section, so much electricity that the same change would take place. This only takes place when we employ Leyden jars; and then the conditions become so complicated that their discussion has hitherto been impossible.

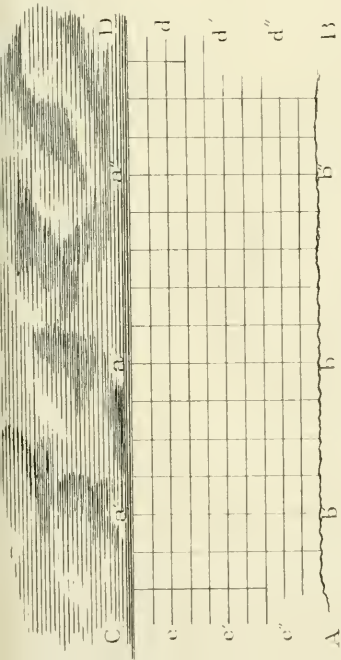
Wüllner*, from a series of careful experiments, has urged objections against the above view of the origin of spectra, and has employed the spectrum of nitrogen as his starting-point. As he had not these separate measurements of the influence of the thickness of the layer, of the intensity of the discharges, of the pressure, and of the quantity of energy given off, it is not possible to follow the discussion of his experiments into details. The appearance of single nitrogen-lines together with the bands may be explained by the reasons given above. An objection formerly raised by me against Wüllner's theory has been set aside by recent experiments of Dewar and Scott†. But that with hydrogen a change of line spectrum into band spectrum cannot be brought about by change of the thickness of the radiating layer may be seen from the remark of Lockyer, that a layer of incandescent hydrogen only a fraction of a millimetre thick in a Geissler's tube shows the same bright lines as the layer in the sun's photosphere thousands of kilometres in thickness. Moreover the gases would have to possess an enormous absorption-coefficient for the luminous rays corresponding to the bands, whereas exactly the opposite follows from the experiments of Gouy‡. Moreover Wüllner has always taken the change of temperature into account.

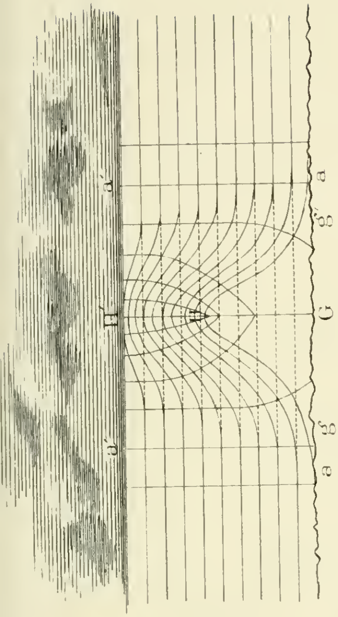
My next problem will be to examine further the region sketched out in this research—namely, to determine the relation between the intensity of the separate lines of the spectrum, the quantities of electricity, and the quantities of energy given

* Wüllner, Wied. *Ann.* viii. p. 590 (1879).

† Dewar and Scott, *Beibl.* iv. p. 309 (1880).

‡ Gouy, *Beibl.* ii. pp. 349, 411 (1878), iii. p. 611 (1879), iv. p. 476 (1880).





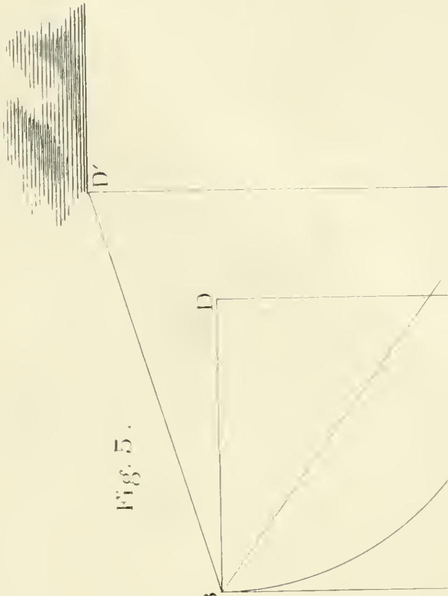


Fig. 5.

Fig. 2

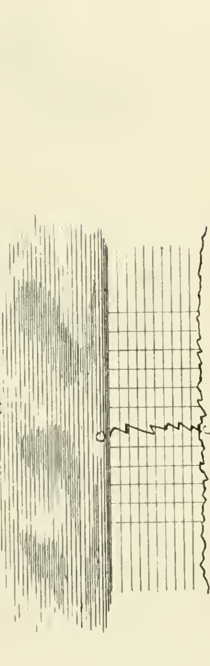
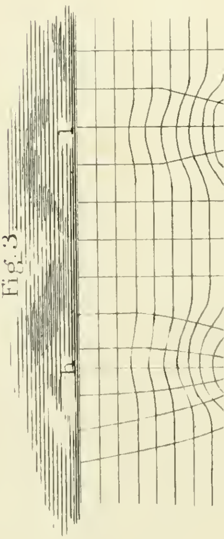


Fig. 3



up. It will in this be necessary first of all to further examine the proposition previously stated, and probably to expand it into the following general statement:—

"In order that definite changes in quality and quantity may take place in the spectrum of a gas produced by electric discharges, equal quantities of energy must be communicated to each molecule, which are, within tolerably wide limits, independent of the pressure of the gas and of the width of the tube."

Leipsic, March 1880.

XLVIII. *On the Space protected by a Lightning-Conductor.* *By* William Henry Preece*.

[Plate X.]

Any portion of non-conducting space disturbed by electricity is called an electric field. At every point of this field, if a small electrified body were placed there, there would be a certain resultant force experienced by it dependent upon the distribution of electricity producing the field. When we know the strength and direction of this resultant force, we know all the properties of the field, and we can express them numerically or delineate them graphically. Faraday (Exp. Res. § 3122 *et seq.*) showed how the distribution of the forces in any electric field can be graphically depicted by drawing lines (which he called *lines of force*) whose direction at every point coincides with the direction of the resultant force at that point; and Clerk Maxwell (Camb. Phil. Trans. 1857) showed how the magnitude of the forces can be indicated by the way in which the lines of force are drawn. The magnitude of the resultant force at any point of the field is a function of the potential at that point; and this potential is measured by the work done in producing the field. The potential at any point is, in fact, measured by the work done in moving a unit of electricity from the point to an infinite distance. Indeed the resultant force at any point is directly proportional to the rate of fall of potential per unit length along the line of force passing through that point. If there be no fall of potential there can be no resultant force; hence if we take any surface in the field such that the potential is the same at every point of the surface, we have what is called an *equipotential surface.* The difference of potential between any two points is called an electromotive force. The lines of force are neces-

* Communicated by the Author.

sarily perpendicular to this surface. When the lines of force and the equipotential surfaces are straight, parallel, and equidistant, we have a *uniform field*. The intensity of the field is shown by the number of lines passing through unit area, and the rate of variation of potential by the number of equipotential surfaces cutting unit length of each line of force. Hence the distances separating the equipotential surfaces are a measure of the electromotive force present. Thus an electric field can be mapped or plotted out so that its properties can be indicated graphically.

The air in an electric field is in a state of tension or strain; and this strain increases along the lines of force with the electromotive force producing it until a limit is reached, when a rent or split occurs in the air along the line of least resistance —which is disruptive discharge, or lightning.

Since the resistance which the air or any other dielectric opposes to this breaking strain is thus limited, there must be a certain rate of fall of potential per unit length which corresponds to this resistance. It follows, therefore, that the number of equipotential surfaces per unit length can represent this limit, or rather the stress which leads to disruptive discharge. Hence we can represent this limit by a length. We can produce disruptive discharge either by approaching the electrified surfaces producing the electric field near to each other, or by increasing the quantity of electricity present upon them; for in each case we should increase the electromotive force and close up, as it were, the equipotential surfaces beyond the limit of resistance. Of course this limit of resistance varies with every dielectric; but we are now dealing only with air at ordinary pressures. It appears from the experiments of Drs. Warren De La Rue and Hugo Müller that the electromotive force determining disruptive discharge in air is about 40,000 volts per centimetre, except for very thin layers of air.

If we take into consideration a flat portion of the earth's surface, A B (Plate X. fig. 1), and assume a highly charged thunder-cloud, C D, floating at some finite distance above it, they would, together with the air, form an electrified system. There would be an electric field; and if we take a small portion of this system, it would be uniform. The lines $a\,b$, $a'\,b'$ ... would be lines of force; and $c\,d$, $c'\,d'$, $c''\,d''$ ... would be equipotential planes.

If the cloud gradually approached the earth's surface (fig. 2), the field would become more intense, the equipotential surfaces would gradually close up, the tension of the air would increase until at last the limit of resistance of the air $e\,f$ would be reached; disruptive discharge would take place, with its atten-

dant thunder and lightning. We can let the line ef represent he limit of resistance of the air if the field be drawn to scale; and we can thus trace the conditions that determine disruptive discharge.

If the earth-surface be not flat but have a hill or a building, as H or L, upon it, then the lines of force and the equipotential planes will be distorted, as shown in fig. 3. If the hill or building be so high as to make the distance H h or L l equal to ef (fig. 2), then we shall again have disruptive discharge.

If instead of a hill or building we erect a solid rod of metal, G H, then the field will be distorted as shown in fig. 4. Now it is quite evident that whatever be the relative distance of the cloud and earth, or whatever be the motion of the cloud, there must be a space $g g'$ along which the lines of force must be longer than $a' a$ or H H′; and hence there must be a circle described around G as a centre which is less subject to disruptive discharge than the space outside the circle; and hence this area may be said to be protected by the rod G H. The same reasoning applies to each equipotential plane; and as each circle diminishes in radius as we ascend, it follows that the rod virtually protects a cone of space whose height is the rod, and whose base is the circle described by the radius G a. It is important to find out what this radius is.

Let us assume that a thunder-cloud is approaching the rod A B (fig. 5) from above, and that it has reached a point D′ where the distance D′ B is equal to the perpendicular height D′ C′. It is evident that if the potential at D be increased until the striking-distance be attained, the line of discharge will be along D′ C or D′ B, and that the length A C′ is under protection. Now the nearer the point D′ is to D the shorter will be the length A C′ under protection; but the minimum length will be A C, since the cloud would never descend lower than the perpendicular distance D C.

Supposing, however, that the cloud had actually descended to D when the discharge took place. Then the latter would strike to the nearest point; and any point within the circumference of the portion of the circle B C (whose radius is D B) would be at a less distance from D than either the point B or the point C.

Hence a lightning-rod protects a conic space whose height is the length of the rod, whose base is a circle having its radius equal to the height of the rod, and whose side is the quadrant of a circle whose radius is equal to the height of the rod.

I have carefully examined every record of accident that I could examine, and I have not yet found one case where damage was inflicted inside this cone when the building was

properly protected. There are many cases where the pinnacles of the same turret of a church have been struck where one has had a rod attached to it; but it is clear that the other pinnacles were outside the cone; and therefore, for protection, each pinnacle should have had its own rod. It is evident also that every prominent point of a building should have its rod, and that the higher the rod the greater is the space protected.

XLIX. *Note on* Mr. E. H. Hall's* *Experiments on the "Action of Magnetism on a permanent Electric Current." By* J. Hopkinson, *F.R.S.*†

IF X, Y, Z be the components of electromotive force, and u, v, w the components of current at any point, in any body conducting electricity, we have the equations

$$\left.\begin{aligned} X &= R_1 u + S_3 v + S_2 w - Tv, \\ Y &= S_3 u + R_2 v + S_1 w + Tu, \\ Z &= S_2 u + S_1 v + R_3 w, \end{aligned}\right\}$$

where R_1, R_2, R_3, S_1, S_2, S_3, T are constants for the substance under its then circumstances (*vide* Maxwell's 'Electricity,' vol. i. p. 349).

After obtaining these equations, Maxwell goes on to say:—"It appears from these equations that we may consider the electromotive force as the resultant of two forces, one of them depending on the coefficients R and S, and the other depending on T alone. The part depending on R and S is related to the current in the same way that the perpendicular on the tangent plane of an ellipsoid is related to the radius vector. The other part, depending on T, is equal to the product of T into the resolved part of the current perpendicular to the axis of T; and its direction is perpendicular to T and to the current, being always in the direction in which the resolved part of the current would lie if turned 90° in the positive direction round T.

"Considering the current and T as vectors, the part of the electromotive force due to T is the vector part of the product T × current.

"The coefficient T may be called the rotatory coefficient. We have reason to believe that it does not exist in any known substance. It should be found, if any where, in magnets which have a polarization in one direction, probably due to a rotational phenomenon in the substance."

* Phil. Mag. March and November 1880.

† Communicated by the Author.

Does not the "rotatory coefficient" of resistance completely express the important facts discovered by Mr. Hall? Instead of expressing these facts by saying that there is a direct action of a magnetic field on a steady current as distinguished from the body conducting the current, may we not with equal convenience express them by saying that the effect of a magnetic field on a conductor is to change its coefficients of resistance in such wise that the electromotive force is no longer a *self-conjugate*-linear-vector function of the current?

L. *On the Number of Electrostatic Units in the Electromagnetic Unit.* *By* R. Shida, *M.E., Imperial College of Engineering, Tokio, Japan*.*

THE object of this paper is to explain measurements made during the month of July last for an evaluation of "v," the number of electrostatic units in the electromagnetic unit —a question which has much engaged the attention of the British Association. We can evaluate "v" by determining the electrostatic and also the electromagnetic measure of any one of the following terms—Electromotive Force, Resistance, Current, Quantity, and Capacity. It is the first of these terms that I measured in the two systems of units; and the E.M.F. was that of Sir William Thomson's gravity Daniell, which is very constant. The question divides itself into two parts:—

(A) *Absolute Electrostatic Measurement of the E.M.F.*

This measurement was made by means of Sir William Thomson's absolute electrometer. It is not easy to explain shortly how the electrostatic measurement is made by this instrument; but, briefly speaking, it is as follows:—Imagine a circular disk suspended by springs in a horizontal plane inside the aperture of another larger plate in the same plane, with a continuous plate below and parallel to them. The force of electrical attraction of the continuous plate on the disk is compared with the gravitating force of a known weight. To effect this, any electrical influence having been entirely removed, a known weight is put on the disk, which is then raised by means of a micrometer-screw until it comes to its original position; and then the weight is taken away, allowing electrical force to act when the continuous plate is adjusted by the aid of another micrometer-screw, to bring the disk to the same position as before. A full account of the instrument will be found in Sir William Thomson's Report on Electro-

* Communicated by Sir William Thomson, having been read in Section A of the Meeting of the British Association at Swansea.

meters (British-Association Report, 1867), and republished along with his other papers on Electrostatics and Magnetism.

In measuring an E.M.F. by this instrument, it is important that the potential of the jar or the guard-ring or disk should be kept constant during the experiment. It was observed, however, that the jar was losing its charge, though very slowly, on account of the pieces of ebonite in the replenisher insulating imperfectly. Of course I could keep the potential of the jar the same during the experiment by means of the replenisher; but I found it very difficult to work the replenisher and to take at the same time accurate readings. For this reason I thought it better, when the experiment is conducted by one experimenter (or, I venture to think, even when there are more experimenters than one), to proceed in the following manner:—First, connect one pole (say zinc) to the continuous plate, and the other pole to the outside of the jar, and take a reading; then reverse the poles and take another reading. Repeat the same operation; that is to say, take a great number of readings by successive reversals. If the experimenter be well practised, the time each reading will take him will be very nearly the same. Let D_1, D_2, D_3, &c. be the readings corresponding to zinc, and D_1', D_2', D_3', &c. be those corresponding to copper; then the difference of the two readings of zinc and copper would be the difference between the mean of any consecutive readings of one pole and the reading of the other taken between those two consecutive readings—such, for example, as $\frac{D_1+D_2}{2}-D_2'$, or $\frac{D_1'+D_2'}{2}-D_2$, &c. Thus we get many values very nearly the same, if not exactly the same, of the true difference in question. If therefore we take the mean of all these, the error due not only to a small loss of charge, but also to a little inaccuracy in the readings, will be avoided. This is the method I used in measuring the E.M.F. of 30 Daniell cells; and the result I obtained is the mean defined as above, $=13{\cdot}283$ divisions of the micrometer screw-head. As regards the mathematical calculation, we have

$$V-V'=2(D-D')\sqrt{\frac{F}{R_1^2+R_2^2}};$$

where $V-V'$ is the E.M.F. of the battery, $D-D'$ the difference of the distances corresponding to the readings of the two poles, F the attracting force of the continuous plate on the disk, R_1 the radius of the disk, and R_2 that of the aperture. Since, now, one division of the micrometer-screw-head corresponds

to a distance of $\frac{5\cdot 08}{10{,}000}$ centim., we get $V-V'=\cdot 90418_7$ (C.G.S.)

The E.M.F. of Thomson's gravity Daniell was measured by comparing it before and after the above experiment directly with that of the above battery by means of Sir William Thomson's quadrant electrometer. The E.M.F., e, of the cell was

$$e=\frac{V-V'}{26\cdot 299}=0\cdot 034380 \text{ C.G.S. electrostatic units.}$$

(B) *Absolute Electromagnetic Measurement of the E.M.F.*

This measurement was made by determining the strength of the current given by the E.M.F. by means of a tangent-galvanometer, and then measuring the resistance of the circuit in the way to be described presently.

The tangent-galvanometer employed consists of a circular coil, of mean radius 18·2 centims., containing 400 turns, in 19 layers, of insulated copper wire, the breadth and the depth of the coil being 2 and 1·3 centims. respectively. The needle of the galvanometer consists of a magnet only about $\frac{1}{2}$ centim. long, made of hard-tempered steel wire, and suspended in the centre of the coil by a single silk fibre. To the needle is attached a very fine straight glass fibre, of such a length that its ends travel round a graduated dial of radius a little less than that of the coil, thus serving for taking readings.

The mathematical theory shows that, in a tangent-galvanometer,

$$c=\frac{H\sqrt{r_0^2+b^2}\tan\alpha}{2\pi n}\cdot\frac{3q^2r_0^2}{3q^2r_0^2\times d^2(q^2-1)}, \quad . \quad . \quad (1)$$

where c is the current-strength, H the horizontal component of earth-magnetism, α the angle of deflection, n the number of turns of wire in the coil, r_0 the mean radius of the coil, b half the breadth of the coil in the plane at right angles to the plane of the coil, d half the depth of the coil in its plane, and q the number of layers in the coil. If E be the E.M.F. producing the current c in a circuit of resistance R, then, by Ohm's law and from the preceding equation, we get

$$E=\frac{RH\sqrt{r_0^2+b^2}\tan\alpha}{2\pi n}\cdot\frac{3q^2r_0^2}{3q^2r_0^2+d^2(q^2-1)}. \quad . \quad . \quad (2)$$

The formula (2) shows that, in order to measure an E.M.F. in absolute electromagnetic units, we have to determine (a) the deflection α, (b) the resistance R, and (c) the horizontal component of earth-magnetism H.

(a) To determine α. The formula (2) also shows that, whatever be the value of R, the product R tan α is a constant quantity as long as E is kept constant; which furnishes this important suggestion—that by varying the resistance R we vary α, and thus get many values very nearly equal, if not equal, of the product R tan α, the mean of which would be the more accurate value of the product. The determination of α, therefore, was performed as follows:—The current from the gravity-cell was passed through the tangent-galvanometer g and a variable resistance r, and the deflection α was noted. The object of introducing the variable resistance is (1) to enable us to alter the resistance R, and (2) to obtain the deflection giving minimum error, which is 45°.

(b) To determine R$(=g+b+r)$. The resistance g of the galvanometer was measured by the Wheatstone's-bridge method, and was equal to 30·86 ohms. The resistance b of the battery was measured by measuring the deflections produced on the scale of Sir William Thomson's quadrant-electrometer by connecting the electrodes of the cell to those of the electrometer, first when the cell was unshunted, and secondly when it was shunted by a known resistance. The resistance b in this case is equal to the product of the difference of the two readings into the shunt, divided by the second reading. It was exactly equal to 2·02 ohms. The corresponding values of a, r, R, so obtained, were as follows:—

a.	r.	R.
45° 15′	80 ohms	107·88
42° 45′	100 „	112·88
51° 39′	50 „	82·88

$$\therefore \text{ the mean value of R tan } \alpha = 104{\cdot}73 \times 10^9.$$

It must, however, be remembered that in all these measurements the ohm, or B.A. unit of resistance, is assumed to be exactly 10^9 C.G.S. units; which is unfortunately doubtful, as was well remarked by Professor Adams, the President of this Section, in his address.

(c) To determine H. The method of determining this element consisted in (1) observing the period of vibration of a magnet under H, and (2) observing the deflection of a magnetometer placed in the magnetic meridian by the action of the magnet placed at a fixed distance in a line at right angles to the magnetic meridian and passing through the centre of the magnetometer. I made the experiment with two different magnets made out of very hard-tempered steel wire, about 0·97 centim. in diameter, and also experimented with each

magnet by varying the distance of the magnet, and found the results to agree very closely with one another. The mean value of H obtained with one magnet is ·15955; and the mean value obtained with the other is ·15937; so that the mean of these two is

$$H = \cdot 15947.$$

The formula used in the calculation of H is

$$H = \frac{2\pi}{t(k-l)(k+l)}\sqrt{\frac{2ki}{\tan\phi}},$$

where t is the period of vibration of magnet under H, k the distance of the centre of the magnet from the magnetometer, l half the length of magnet, i the moment of inertia of the magnet, and ϕ the angle of deflection of the magnetometer.

We have now come to the evaluation of "v." The formula (2) gives

$$\epsilon = 1{\cdot}01172 \times 10^8 \text{ (C.G.S.) electromagnetic units.}$$

Hence

$$v = 294{\cdot}4 \times 10^8 \text{ centims. per second,}$$

which agrees well with the latest value obtained by Sir William Thomson, namely 293×10^8.

Although I took as much care as possible in making all the above measurements leading to this evaluation of "v," yet since, from want of time, it was only on one occasion that I was able to make the complete measurements, there may have been some cause or causes of error unnoticed. I intend therefore to repeat the whole experiment, and hope to be able to make a further communication.

In conclusion, I must say (and I say with extreme gratitude) that if the experiment be in any way satisfactory, it is chiefly due to the very able and kind instructions given me by Sir William Thomson and his assistants in carrying it out.

Addition, Nov. 18, 1880.—These experiments have, since the communication of the above paper to the British Association, been several times very carefully repeated, with in every case a confirmation of the close accuracy of the determination of the electrostatic value of electromotive force. In the electromagnetic determination, however, a correction has been made for the torsion of the single silk fibre by which the needle of the tangent-galvanometer was suspended. As it was supposed that the torsion of a single fibre of silk might be neglected, no correction was made in the first results; but the

error due to this cause has in these later experiments been determined and allowed for. Five experiments were made, and the corresponding values of "v" calculated from the results. These values are as follows:—

$$\begin{array}{c} v. \\ 299{\cdot}9 \times 10^8 \\ 300{\cdot}3 \times 10^8 \\ 299{\cdot}4 \times 10^8 \\ 298{\cdot}0 \times 10^8 \\ 299{\cdot}9 \times 10^8 \end{array}$$

Mean value . . . $299{\cdot}5 \times 10^8$.

LI. *Note on the Relation between the Mechanical Equivalent of Heat and the Ohm. By* L. B. FLETCHER, *Student in Physics, Johns Hopkins University**.

A SINGULAR error occurs in a paper, published in the Philosophical Magazine (April and May 1880), by Dr. C. R. A. Wright. After remarking that Joule's value of the mechanical equivalent of heat, derived from experiments on the heat generated by a measured current in a wire of known resistance, is probably, when corrected for the error in the resistance-estimation due to superheating of the wire, from 1·5 to 2 per cent. higher than Joule's water-friction value, Dr. Wright goes on to say (p. 264) that:—"This difference between the two values is precisely that which would subsist did an error to an equal amount exist in the B.A. resistance-unit valuation: *i. e.* if the B.A. unit were 1·015 to 1·020 earth-quadrant per second instead of being exactly 1 earth-quadrant per second, the value of J deduced from Joule's 1867 experiments would be 1·015 to 1·020 times the true value; for it is calculated by the formula

$$J = \frac{C^2 R t}{H},$$

where C is the current, R the resistance, t the time, and H the heat evolved." This statement is evidently incorrect; for if the ohm is really 1·02 earth-quadrant per second, and was assumed by Joule to be exactly 1·00 earth-quadrant per second, Joule's value for the resistance of his wire, and consequently his value for J obtained by this method, must be 2 per cent. too *small*.

* Communicated by the Author.

The fact, therefore, that Joule's value of J obtained by this method is too *large* (assnming his water-friction value to be correct), points to the conclusion that the ohm is really *smaller* than it was intended to be, in accordance with the results of Rowland* and Lorenz†, and in direct contradiction to that of Kohlrausch. As the erroneous statement of Dr. Wright has apparently been confirmed by a redetermination of J, recently communicated by him to the Physical Society, and has, further, been given wide publicity by a reference in the address of Prof. W. Grylls Adams before the British Association, I am unwilling to allow it to pass any longer unchallenged. I have been engaged for the greater part of a year in a redetermination of J by the electrical method, avoiding the error due to superheating; and the results so far obtained confirm the supposition that the ohm is smaller than 1 earth-quadrant per second, although considerable time must elapse before I can publish the exact amount of its error.

Baltimore, November 8, 1880.

LII. *On Action at a Distance. By* WALTER R. BROWNE, *M.A., M. Inst. C. E., late Fellow of Trinity College, Cambridge* ‡.

THE object of this paper is partly historical, partly critical. In discussing what is called "Action at a Distance," the statement is frequently made that Newton was of opinion that "nobody who possessed a competent faculty of thinking" could possibly imagine such a thing to exist. The writer wishes, first, to show historically that this is by no means an accurate representation of Newton's views, and, secondly, to consider critically whether the repudiation of "action at a distance," which is now certainly common, is, after all, justified by the facts of the universe.

In the first place, Newton's words, contained in the Third Letter to Bentley, are as follows:—"That gravity should be innate, inherent, and essential to matter, so that one body may act on another body at a distance through a vacuum, without the mediation of any thing else by and through which their action and force may be conveyed from one to the other, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to fixed laws; but whether this agent be

* Am. Jour. Sci. and Arts, April 1878 (vol. xv.).

† Pogg. *Ann.* Bd. cxlix. (1873) p. 251.

‡ Communicated by the Physical Society, having been read at the Meeting on November 13.

material or immaterial I have left to the consideration of my readers."

Now, in speaking of this passage, it is usual to quote the first of these sentences only, and omit the second; and yet it is obvious that the second is intended to explain and define the sense of the first. Read by the light of the second, it seems perfectly clear that all which is denied in the first is the possibility of gravity being an inherent property of matter, in the sense in which hardness, inertia, &c. may be considered as properties. What Newton might seem to have had in his mind was the coarse materialism of Democritus and Lucretius, which held that all the phenomena of the universe were due to the mere motions and clashings of its ponderable atoms. This at least, he would hold, was disproved by his discoveries, because to defend it by assuming an occult property of matter, which could extend to a distance, was absurd. All, however, which he really says is that one body cannot, *uncaused*, act on another at a distance. In the second sentence he expressly uses, not indeed the word Cause, but the much stronger word Agent; and he distinctly contemplates the possibility of this agent being, not material, but immaterial. It seems clear, therefore, that he is thinking of nothing less than of denying that action at a distance may, as a matter of fact, exist. Indeed, when we consider that this passage occurs, not in a mathematical work, but in a letter expressly treating of the relation between the discoveries of science and the doctrines of theology, and when we remember the strong theological views which he is known to have held, it seems impossible to doubt that he would have been perfectly contented to acquiesce in the immaterial nature of the agent of gravity; though, no doubt, he would have been perfectly open to consider any reasonable hypothesis of a material agent which might have been placed before him.

Having thus attempted to restore the true sense of this famous passage, the writer will go on to consider, in the second place, how far the conception of Action at a distance actually merits the condemnation it has received. It seems desirable to commence with a definition, and to lay down the consequences which flow from it in general, before proceeding to consider particular cases.

Definition.—" By the term 'Action at a distance' is meant that direct action takes place between two bodies, separated from each other by a finite distance, without the intervention of any other body whatever."

Now if action at a distance does not exist, then the only *direct* way in which one body (A) can act upon another (B) is by coming into absolute contact with it; and the only *indi-*

rect ways in which it can act upon it are two, viz. either by projecting a third body from contact with itself into contact with B, or by diverting some third body which, if not diverted, would have come into contact with B.

If action at a distance does not exist, all the actions between all the bodies of the universe must be explicable, by impact, on one of these three hypotheses. If any phenomenon takes place which cannot be so explained, then action at a distance does exist. It may be added that, if it is shown to exist in any one instance and at any distance, there is no probability against its existence in any other instance and at any other distance. It is no less wonderful, and no more wonderful, that two bodies should act on each other across the hundred millionth of an inch, than that they should act on each other across a hundred million of miles. In fact it is easy to conceive a creature so large, or so small, that the difference between these two distances would appear to it quite insignificant.

Let us now take the above three hypotheses and see whether all the actions in the universe can be explained by them.

First, as to the direct impact of one interacting body A upon another B. This may no doubt explain certain obvious cases, as the stoppage of a falling body when it reaches the earth; but it is equally obvious that there are many others, such as gravity, magnetism, &c., which it cannot explain. In fact, it will be granted that in these and many other cases there is an *apparent* action between bodies at a distance; and our business is to see whether it is real or apparent only.

Secondly, with reference to the projection of other bodies from A against B. It is clear that the actions thus produced can be actions of *repulsion* only: therefore this principle cannot explain any case of attraction. Moreover the power by which A is able to project these bodies against B itself requires explanation. If they have previously been at rest in relation to A, then A can only project them by some innate explosive power totally different from impact. And if any one suggests that the bodies have previously been in motion with respect to A, and that they are projected by elastic reaction from A, then he must be asked to give an explanation of elasticity from impact only, and without introducing action at a distance. In any case it seems clear that this principle will not carry us very far in explaining the actions of the universe.

Thirdly, we have the principle that A may stop certain other bodies, which would otherwise have impinged upon B. This principle, as is well known, was applied by Le Sage to explain the facts of gravitation *. His hypothesis was that

* Sir W. Thomson, Phil. Mag. May 1873.

showers of "extramundane particles" are sweeping through space equally in all directions, and that a fraction of these, being intercepted by A and B, urge those two bodies towards each other. This hypothesis is encumbered with a large number of arbitrary assumptions; and the latest supporter of the theory, Mr. S. Tolver Preston*, presents it under a greatly modified form. He supposes the solar system to be immersed in an impalpable gas, the particles of which have a mean length of free path greater than the distance through which gravity has been observed to hold (greater, therefore, than the distance between the Sun and Neptune), and which tend to bring together, by the resultant of their impacts upon them, any two bodies within that range. It is not proposed in this paper to attempt an exhaustive discussion of this theory; but were it left as an unquestioned explanation of gravitation, it might be thought a strong presumption that all other actions were to be explained on the same principle. It may therefore be remarked that it is encumbered by very serious difficulties. In addition to those put forward by Dr. Croll and others, the following may be suggested:—

(1) Mr. Tolver Preston founds his theory on the late Prof. Maxwell's proof †, that "a self-acting adjustment goes on among a system of bodies or particles in free collision, such that the particles are caused to move equally in all directions, *this being the condition requisite to produce equilibrium of pressure*" ‡. Now this equilibrium of pressure, and the theory based upon it, may be perfectly true for all known gases. But all such gases are under certain conditions, which need not hold universally; in especial they are *bounded* in some way. The atmosphere, which is the freest gas we can observe directly, is bounded by the earth on one side and space on the other, and is prevented from passing into space by the action of gravity. But we have no right whatever to assume such a boundary for interstellar space, or to assume that a gas filling such a space would have equilibrium of pressure. The probability would seem to be the other way; for any disturbance in such a gas would tend to propagate itself in all directions for ever. In any case, Maxwell's results must be proved, not assumed, to hold for this gravity-gas, as it may be termed.

(2) Another difficulty in the theory is the enormous degree of porosity which it postulates for solid bodies. To fix our ideas, suppose that, in any unit of surface of a solid, one mil-

* Phil. Mag. Sept. 1877, Nov. 1877, Jan. 1878.

† S. Tolver Preston, Phil. Mag. Sept. 1877.

‡ I have, unfortunately, failed to verify the reference to this paper of Prof. Maxwell's, given by Mr. Tolver Preston, and therefore can speak of it only from his description.

lionth part only is occupied by the really solid part (*i. e.* the part which would stop the particles of the gravity-gas) of the molecules composing that surface. Then it is obvious that a layer of such molecules a few millions thick would be practically certain to stop the whole of the gravity-particles impinging upon it. No arrangement of the molecules one behind the other will get over this, because the gravity-particles are assumed to come in all directions at once. Now such a layer would certainly be no more than a small fraction of an inch in thickness. And yet it is absolutely necessary for the theory (in order to explain how gravity varies as mass) to suppose that these gravity-particles pass through the 16,000 miles of the earth's diameter, under the enormous density, pressure, and temperature which must exist in the interior, without having more than a very small proportion of their number stopped in the passage. The difficulty is rendered the greater when we remember that, *ex hyp.*, these attenuated molecules cannot act on each other at a distance, in producing the various phenomena of solid bodies, but only in one of the three modes of direct impact enumerated above.

(3) Another difficulty arises from the fact that the heavenly bodies are not found to experience any perceptible resistance whatever in passing through this gravity-gas. It is clear that if a body be in motion in the midst of a shower of such particles coming equally from all directions, it will receive a greater number of blows on its front surface, and a less number on its rear surface, than if it were at rest; and consequently its motion will be retarded. The only way of surmounting this difficulty is to suppose that the heavenly bodies, in relation to the gravity-gas, are practically at rest; in other words, that the velocity of the gravity-particles is practically indefinite compared with that of the heavenly bodies. Since in the case of Mercury, for instance, this latter velocity is about thirty miles per second, it is clear that the velocity of the gravity-particles must be something altogether beyond calculation; and then, since the effect of the collisions is, after all, very limited, the mass of the particles must be assumed correspondingly small. Hence our conception of the gravity-gas must practically be that of an indefinite number of indefinitely small particles moving in all directions with indefinitely high velocities—a conception from which it hardly seems safe to draw any definite conclusion whatever.

(4) The last-mentioned difficulty leads to another, viz. to fix the relations between the gravity-gas and the luminiferous æther. They cannot be the same; for Mr. Tolver Preston and Prof. Maxwell have shown * that the velocity of propagation

* S. Tolver Preston, Phil. Mag. June 1877.

of a wave in such a gas $= \frac{\sqrt{5}}{3} \times$ the velocity of the gas-particles. Since the velocity of waves in the æther is about 180,000 miles per second, this would give the velocity of the particles themselves = about 130,000 miles per second—a velocity immensely below what is required to account for the fact of non-resistance. But if the æther and the gravity-gas be different bodies, the particles of the latter must be colliding continually with those of the former, as they collide with the molecules of ordinary matter. How is it that no effects due to such collisions are observed? It would seem likely that they would assume the shape of a diffused glow of light and heat, growing more and more intense as the translatory motion of the gravity-particles was turned into vibratory motion of the æther-particles. It is needless to say that nothing in the least resembling this takes place.

We will here leave the discussion of Le Sage's impact theory, as explaining the particular case of gravitation, and go on to inquire how the same, or any other impact theory, can explain some other phenomena of the universe. We will first take those of cohesion.

Cohesion.—To fix our ideas, let us take the case of a square bar of good wrought iron or mild steel, 1 foot long and 1 square inch in area. Then the following two facts, amongst others, have to be accounted for:—

(*a*) The extension of the bar as a whole (and therefore the extension of the mean distance between the successive layers of its molecules) by $\frac{1}{1000}$ of its length is sufficient to produce between the successive sections of the bar a stress of tension (taking the form of an attraction between the sections) of about 15 tons, say 8000 times the attraction exercised by the earth upon the whole bar when placed in contact with it.

(*b*) The contraction of the bar through the same relative distance is sufficient to produce between the sections a stress of compression (taking the form of a repulsion between the sections) also of 15 tons or thereabouts.

Can these two facts be explained on any of the three impact theories, which we have shown to be the only possible ones? It seems almost sufficient to ask the question; but it may be well to take them in order.

(1) Can the facts be explained on the hypothesis of direct contact between the molecules? Were this true, it would be impossible to produce any contraction of the bar without forcing two solid bodies into the same space. It is obvious that it will not do to suggest that the contraction may be in the molecules themselves; for then we have only to transfer the inquiry to the particles composing those molecules. Are these

particles themselves in contact or not? If they are not, they cannot keep the bar together; if they are, they cannot be compressed. Again, if the molecules are spherical, or of any other regular shape whatever, they cannot oppose any resistance to separation, *i. e.* there can be no tensional stress. The only way out of this seems to be to conceive them shaped something like burrs, and holding on to each other by hooks. This is altogether contrary to the vortex-atom and all other known theories of molecules. Moreover such burr-like molecules must hold to each other somewhat loosely; and a certain amount of extension would be necessary (as in the case of a slack chain) before any resistance was experienced. But no such slackness has been observed with the most delicate instruments; and we have seen that an extension of $\frac{1}{1000}$ is sufficient to produce an enormous resistance. For these and the like reasons the hypothesis of direct contact is inadmissible.

(2) Can the facts be explained on the hypothesis of particles projected from the molecules of one section against those of the next? Now it is clear that any effect due to this cause will be merely an effect of repulsion. Consequently the end section of the bar will be repelled from that next to it, and will fly off; another body brought into contact with the bar will be repelled by it, &c. For these and the like reasons this hypothesis is inadmissible.

(3) Can the facts be explained on the same hypothesis as that of Le Sage, viz. of independent particles flying through space and intercepted by the molecules of the bar? In the first place, it is clear that these cannot be the particles of the gravity-gas; for if these pass through the earth without having more than a small proportion stopped, it is clear that the number intercepted by an inch of iron will be infinitesimal. We should have to conceive, therefore, a separate atmosphere for each solid body, and an atmosphere the effects of which are many thousand times as great as that of the gravity-gas. But, further, let us assume this atmosphere, and consider what will happen when the bar is extended. Any one section will be removed to a greater distance from the next, and its sheltering influence will be diminished in the inverse ratio of the squares of the distances. Consequently the effect of extension will be to diminish the attraction between the sections; whereas the actual effect is enormously to increase it. For these and the like reasons this third and last hypothesis is also inadmissible.

The two latter hypotheses, and any combination of them, labour under a further and fatal disadvantage, viz. that the cohesion of the bar would be different in different parts. Thus in whatever way the flying particles are supposed to move, it

is evident by symmetry that the central section will be solicited in one direction precisely as much as in another; hence the slightest pull will cause the bar to part in the middle.

The above trains of reasoning are not long, and rest on undoubted facts; and the writer has not been able to discover any flaw in them. But unless some such flaw, and a fatal one, be discovered, it must be held to be demonstrated that the phenomena of cohesion cannot be explained except on the hypothesis of action at a distance.

Magnetism.—Of the many difficult cases presented by the phenomena of electricity, it will be sufficient to cite one of the simplest. When an ordinary iron magnet is brought near a piece of iron, the latter is attracted to it. Now the first impact hypothesis is here inadmissible, because the bodies are not in contact; and the second, because the effect is one of attraction, not of repulsion. Thus the only possible explanation of this fact, apart from action at a distance, is by supposing that the magnet intercepts a proportion of a shower of particles which would otherwise impinge equally in all directions upon the iron. It is of course possible to imagine a "magnetism-gas," different again from both the "gravity-gas" and the "cohesion-gases," to which this would apply; but the writer has not been able to imagine any property, consistent with the principle of impact, which could be given to the magnet, such as to make it intercept these particles, when the same magnet, before being magnetized, would be unable to do so—and also such as would make it intercept the particles flying towards a piece of iron, and not to intercept the particles flying towards a similar piece of brass.

Vibrations.—Any thing like the vibration of a molecule about a central position (which is the fundamental idea in explaining Heat, Light, and all undulatory movements) seems to be impossible on this theory. For a molecule, once started, is in the position of a free projectile through space, and will continue to move in a straight line until it accidentally strikes against some other molecule which may be moving in any other direction. Hence it is obvious that the chance of the molecule ever coming back to its original position is indefinitely small. This applies especially to the case of the æther, the particles of which are comparable to those of the gravity-gas.

The above are a few very simple cases, in which it seems certainly difficult to avoid the conclusion that action at a distance must necessarily exist. And if it exists in these cases, then, as already remarked, it becomes at least probable that it may exist in other cases, such as gravity, where the evidence

is not so clear. In conclusion it may be asked, therefore, what real reason is there why this hypothesis of action at a distance should not be admitted. To some minds it seems to present itself in the light of a theory which it is *à priori* difficult, if not impossible, to believe. But Physics has nothing to do with mental impressions; and in the history of the Inductive Sciences there are many well-known instances, where *à priori* notions of this kind have seriously hindered the advance of knowledge. It is evident that the progress of science in any direction must be towards certain universal and final facts, beyond which she cannot go. On the one theory, the ultimate fact in the case of gravity is enunciated in a very simple law of force, connecting together all ponderable bodies. On the other theory, the ultimate facts are apparently enunciated in the laws of impact between elastic bodies (which also involve the conception of force), and in the statement of the fundamental conceptions and results of the Kinetic Theory of Gases, assumed to hold for an exceedingly rare gas pervading all space. The writer submits that, *à priori*, one of these theories is as likely as the other—but that both must be judged by the test of their accordance with known facts, and by that test alone must be accepted or condemned.

On the general comparison of the two views, as to their power of explaining facts, one remark may perhaps be allowed. It will not, probably, be denied that, if we only knew the exact laws of any action whatever between bodies, we could at once explain it on the hypothesis that these bodies are made up of centres of force, each possessing position and inertia, and acting on the other centres according to laws which it would be easy, or at least possible, to determine. It certainly cannot be said at present that we could equally explain any action by the mere laws of impact, even if we include in them those of elasticity. So long as these two statements hold, it seems more in accordance with the cautious spirit of true science to maintain the old theory, than unreservedly to adopt the new one.

LIII. *Notices respecting New Books.*

Reports on the Geology of Queensland. By ROBERT L. JACK, *F.G.S. &c. With Woodcuts, Plates, and Maps.* 4to. Brisbane, 1879.

THESE official Reports, three in number, presented to The Honourable the Minister for Mines, Brisbane, by the Geological Surveyor for Northern Queensland, are of considerable interest, especially as regards the occurrence and supply of Coal and Gold in some parts of that region. The first is a Report on the Geology and Mineral Resources of the district between the

Charters-Towers Gold-fields and the Coast, dated Townsville, 14th May, 1878, with a Supplement dated 10th November. The formations observed were :—1. Recent Alluvium, with sandstones, grits, breccias, and conglomerates, rarely associated with organic remains, and covering auriferous " leads " (old channels). 2. Tertiary Basalt. 3. A Sandstone of undetermined age, but older than the " Desert Sandstone " of Daintree. 4. Devonian formations, in three stages, more or less fossiliferous. 5. Predevonian metamorphic rocks, of wide extent, and carrying all the metalliferous veins, dykes, or reefs of the district. The richest gold-veins are in the most highly metamorphosed rocks near their contact with those least altered ; and the principal of these auriferous reefs have a concentric arrangement between Charters-Towers and Millchester (fig. 26). These greywackes, quartzites, shales, slates, micaceous and other schists, gneiss, granites, syenites, and porphyries are carefully noted, according to localities, as well as their dykes of porphyry, diorite, quartz, &c. Copper, tin, galena, blende, and pyrites occur in variable quantities in some of the porphyritic and other reefs, some of which are illustrated in neat woodcut sections. The gold is not quite so good as that of Victoria and New South Wales, but is abundant. Statistics of the yield and value are duly given.

The second Report, dated 7th June, 1878, is on the Geological features of part of the Coast-range between the Dalrymple and Charters-Towers Roads. This covers a blank in the geological map of the former Report. Granite, sandstone, greywacke, and limestone, more or less altered, and referable to the series before mentioned, though difficult of exact determination, were here met with.

The third Report, dated November 23rd, 1878, is on the Northern part of the Bowen-River Coalfield, with a note of errata dated Nov. 10th, 1879. This coalfield, stretching from the Bowen River "through six degrees of latitude to the heads of the Dawson," has long been known, chiefly from the late Mr. Daintree's researches and memoirs. The northern end dies out on wide-spread metamorphic rocks of Precarboniferous age, which bear some patches of more recent formations here and there. On the slopes of the Herbert and Clarke Ranges (plutonic and metamorphic) lie volcanic agglomerates ; the lower series of the coalfield, consisting of sandstones and conglomerates, succeed ; then the bedded porphyrites and basalts of Mount Toussaint and Mount Macedon ; then the middle series (marine) of the coalfield, with fossiliferous sandstones (*Productus* &c.), carbonaceous shales, and two seams of coal : altogether about 1848 feet thick. The upper (mainly freshwater) coal series, at least 1000 feet thick, succeeds, with *Glossopteris* &c., and a bed with *Goniatites* and *Productus*. This coal is very much burnt and destroyed by overlying and intrusive lavas.

Notes on the quality and value of the respective Coal-seams are given ; also careful lists of references to papers and reports of other observers bearing on the history and nature of these coal-beds.

These Brisbane Reports are evidently the work of a strictly

educated geologist, making careful observations, and bringing a well-trained and philosophic mind to grasp the details and compare them with the experiences of others. Though not artistic, his rough sketches of scenery are useful; for he knows how to indicate the real structure of the country.

LIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 134.]

November 3, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Serpentine and Associated Rocks of Anglesey, with a Note on the so-called Serpentine of Porthdinlleyn (Caernarvonshire)." By Prof. T. G. Bonney, M.A., F.R.S., Sec. G.S.

Several patches of serpentine are indicated on the Geological-Survey map on the western side of Anglesey, near Tre-Valley station, and a considerable one on Holyhead island near Rhoscolyn. These really include three very distinct varieties of rocks—(1) compact green schistose rocks, (2) gabbro, (3) true serpentine. The author described the mode of occurrence of each of these, and their relations, the serpentine being almost certainly intrusive in the schist, and the gabbro in the serpentine. The microscopic structure of the various rocks was described in detail, especially of the last. It presents the usual characteristics, and is an altered olivine rock which has contained bronzite. One or two varieties are rather peculiar; an ophicalcite and a compact chloritic schist containing chromite are also noticed. At Porthdinlleyn there is no serpentine, but an interesting series of agglomerates and (probably) lava-flows of a basic nature, which may now be denominated diabases.

2. "Note on the Occurrence of Remains of Recent Plants in Brown Iron-ore." By J. Arthur Phillips, Esq., F.G.S.

3. "Notes on the Locality of some Fossils found in the Carboniferous rocks at T'ang Shan, in the province of Chih Li, China." By James W. Carrall, Esq., F.G.S. With a Note by Wm. Carruthers, Esq., F.R.S., F.G.S.

November 17, 1880.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On Abnormal Geological Deposits in the Bristol District." By Charles Moore, Esq., F.G.S.

The author remarked that the Frome district shows numerous unconformable Secondary deposits and "vein-fissures" resting upon or passing down through the Carboniferous Limestone, as described in his former paper (Quart. Journ. Geol. Soc. vol. xxiii. p. 449). He gave some further particulars as to these deposits, and especially described the occurrence of Postpliocene, Liassic, and Rhætic deposits in the *Microlestes*-quarry near Shepton-Mallet. Here the

lower part of a fissure is filled with a brown marl containing crystals of carbonate of lime and numerous remains of *Arvicolæ*, Frogs, Birds, and Fishes. The jaws of *Arvicola* were very abundant.

He then proceeded to describe the occurrence of similar phenomena in the Bristol area, as at Durdham and Clifton Downs, in the gorge of the Avon at Clifton, at Ashton and Westbury-on-Trym, in the Yate rock, in Nettlebury quarry, at Clevedon, and on the Thornbury railway. He noticed the occurrence in the infillings of fissures traversing the Carboniferous Limestone of these localities of fossil remains belonging to various geological ages; and he especially called attention to the presence in different deposits of an immense number of small tubular bodies of doubtful origin, for which, should they prove to be of organic nature, he proposed the name of *Tubulella ambigua*. By different authorities these little bodies have been assimilated to Serpulæ (*Filograna*), insect-tubes, and the casts of the fine roots of plants. With regard to the age of the fissure-deposits, the author remarked that although in some fissures the infilling shows a mixture of organisms, in most cases each "vein" appears to have an individuality of its own, and thus the veins represent intervals of geological time clearly distinct from one another, different fissures showing infillings of Alluvium, Oolite, Lias, Rhætic, and Keuper beds. The presence of his *Tubulella* he considered to indicate freshwater conditions.

The author also referred to the discovery of *Thecodontosaurus* and *Palæosaurus* many years ago at the edge of Durdham Down, and discussed the age of the deposit containing them, which was originally supposed to be Permian, and was referred by Mr. Etheridge to the Dolomitic Conglomerate at the base of the Keuper. The author stated that he had found remains of the same genera in Rhætic deposits at Holwell and Clifton Down, and had hence been led to refer the two genera to that age. He stated, however, that he had since discovered teeth of *Thecodontosaurus* identical with those of the Bristol area in a deposit belonging to the middle of the Upper Keuper at Ruishton near Taunton, and recognized certain differences between these teeth and those of the same genus from the Rhætic beds of Holwell; hence he was led to give up the notion that the former were of Rhætic age, and to refer them to the Upper Keuper; but he remarked upon the interesting fact that, while most of the generic forms of the Keuper are represented in the Rhætic, the species differ.

2. "Interglacial Deposits of West Cumberland and North Lancashire." By J. D. Kendall, Esq., C.E., F.G.S.

The glacial deposits of the district consist of an Upper and a Lower Boulder-clay, with an intercalated group of sand, gravel, and clay, the three being rarely present in one section. These deposits occur fairly continuously up to 500 feet above the sea, and in patches up to 1000 feet. Associated with these glacial beds are deposits of vegetable matter, which, when occurring on the seashore, have been designated submerged forests. The author considers this designation incorrect. The results of a large number of

borings at Lindal, in Furness, are given, in which, beneath Upper Boulder-clay, one of these vegetable deposits was pierced, resting on Boulder-clays or sand. Similar deposits (which have been less completely examined) occur at Crossgates, Walney Island, and Drigg. Another is near St. Bees, which has been more minutely examined; and yet another near Maryport. These deposits are not, like the Lindal beds, clearly interglacial, but, being compact and in other ways differing from the ordinary peaty deposits, are believed by the author to be so; further, they all rest on *Lower* Boulder-clay.

The author believes that the vegetable matter was not produced *in situ*, but accumulated under water. Rootstocks certainly occur in position of growth; but their roots do not pass down into the underlying Boulder-clay; so they may have floated into this position. The author considers this to throw light on the formation of coal.

LV. *Intelligence and Miscellaneous Articles.*

THE DISCOVERY OF OXIDE OF ANTIMONY IN EXTENSIVE LODES IN SONORA, MEXICO. BY E. T. COX, OF TUCSON, ARIZONA TERRITORY.

UP to the present time the antimony of commerce has been mostly obtained by the reduction of the sulphide; and though this ore is widely distributed over the globe, it is, as a rule, associated with a variety of mineral substances that obstruct reduction and add to the cost of purifying the metal. These sulphides are also found in such sparse quantities, that the metal usually commands from three to four times the price of lead, and fully as much as that of tin or copper. At present the supply of sulphides of antimony for the English smelters is obtained from Algeria, Spain, and Ceylon. Small quantities of oxide-of-antimony ores have been found in portions of Europe and in Ceylon, but at no time in such quantities as to elicit special attention. When, therefore, about a year ago, I called the attention of English metallurgists and smelters to the occurrence of vast lodes of almost pure oxide of antimony in the district of Altar, Sonora, Mexico, thirty miles from the Gulf of California, it seemed too marvellous for their belief. A company of gentlemen of Boston, Mass., now have control of these antimony-mines; and the ore will soon be in the hands of smelters.

The geological features of the country where this ore abounds are similar to those of Southern Arizona. The mountains are in short, narrow ranges, having for the most part a northerly and southerly trend. Their crests are either rugged or well-rounded cones, according to the nature of the rocks forming their mass. Between these ranges we have what is called mesa or tableland; the latter is formed of the débris of the mountains. This material is of so loose and porous a nature, that the small amount of rain which falls sinks through it and leaves the land dry and arid. As far as I have been able to make out the order of the rocks forming

these mountain chains, we have first granite; and this is flanked by Subcarboniferous limestone, in most places so crystalline as to obliterate all traces of fossils. Protruding through these, and forming the mountain-peaks, we have porphyry, quartzites, basalt, diorites, and trachytes.

The country rock in the immediate vicinity of the antimony-mines is quartzite and limestone. The lodes are from four to twenty feet wide; and exploitation work, carried to a depth of thirty feet, shows that the fissures are filled from wall to wall with the oxide of antimony, almost pure and remarkably uniform in character. The course of the lodes is nearly north and south; the pitch is high to the east. The area over which the ore is found may be roughly stated to be five or six miles long and half a mile or more wide.

The Boston Company controls nine mines, each of which is a full Mexican claim, 800 metres (2624 feet 8 inches) long and 200 metres (656 feet 2 inches) wide. On three of the mines the crop, which is solid oxide of antimony, stands up boldly above the general surface, and may be traced along the claims for many hundred feet. As stated above, the ore, so far as explorations have exposed it, is almost pure oxide of antimony, the little impurity it contains being silica. The fire assays show it to contain from 60 per cent. to 70 per cent. of pure metal; and I have estimated the entire lode to average 50 per cent. By selection the average may be augmented. On going down to a greater depth in the lode, it is possible that the oxides may give place to sulphides; but thus far there is not the slightest evidence of any change.

This discovery is destined to produce a marked influence upon the production of metallic antimony, and to greatly extend its uses.

Prof. S. P. Sharples, of Boston, after an examination of many specimens of the oxide of antimony received from me, has made the following statement:—"The mineral varies in colour from almost white to a very dark brown. The specific gravity of one of the purest specimens is 5·07; and it contained 5 per cent. of water, and 75 per cent. of antimony. This composition and specific gravity approach very closely the same for *stibiconite.*

"The mineral is only very slightly soluble in hydrochloric or nitric acid, or aqua regia. Fusion with bisulphate of soda only partially resolves it. It is, however, readily and easily decomposed in a platinum crucible with carbonate of soda.

"This oxide of antimony has hitherto been found only as a slight coating on other antimony minerals; and it has been difficult to get specimens of it even a few grains in weight.

"The mineral is not easily reduced before the blowpipe, but is very easily reduced in a crucible with powdered charcoal or cyanide of potassium, giving at a single operation buttons of star antimony."—*American Journal of Science*, November 1880.

ON THE THERMOELECTRIC POWER OF IRON AND PLATINUM IN VACUO. BY PROF. C. A. YOUNG, OF PRINCETON, N. J.

Exner, a few months ago, published a paper asserting that the

thermoelectric power of antimony and bismuth is destroyed by removing them from all contact with oxygen and immersing them in an atmosphere of pure nitrogen. From this he argues that the thermoelectric force in general is due to the contact of the gases which bathe the metals. The following experiment was tried to test the theory:—

By the kindness of Mr. Edison and Mr. Upton a vacuum-tube was prepared in Mr. Edison's laboratory, containing an iron wire, about two inches long, firmly joined to two platinum terminals which passed through the walls of the tube. The tube was exhausted until the spark from a two-inch induction-coil would not pass $\frac{1}{8}$ of an inch in the gauge-tube, indicating a residual atmosphere of about one-millionth. The wire was heated to incandescence during the exhaustion, in order to drive off any possible occluded gases. The platinum wires outside the tubes were joined to iron wires, the joinings being covered by glass tubes slipped over them; and a sensitive reflecting galvanometer was included in the circuit. By laying the tube and connected joinings in the sunshine, and alternately shading one or several of the joinings, it was found that the electromotive power of the joinings within the tube was precisely the same as that of those without, and the development of current just as rapid. There was no trace of any modification due to the exhaustion.—*American Journal of Science*, November 1880.

ON INDUCTION IN ROTATING SPHERES. BY H. HERTZ*.

The reciprocal actions between magnets and rotating masses of metals, discovered by Arago, were first apprehended by Faraday as phenomena of electrodynamic attraction, and traced to currents which are induced in the masses by the magnets.

In the present work the problem, to determine these currents from the mathematical theory, is solved for the case that the body considered is a solid or a hollow sphere rotating about a diameter. The inducing magnets can be situated in the external or, with hollow spheres, in the internal space. The solution is also extended to the case in which the mass of the sphere is capable of assuming magnetic polarity. The principal results are collected in the following summary; for the workings I must refer the reader to the original.

As in the sphere electrical motions can be superposed, the inducing-potential function is resolved according to spherical functions, and a term of the evolution of the form $A\rho^n \cos i\,\omega P_m(\theta)$ considered. For the induction produced by such a term the following propositions were demonstrated:—

The flow always takes place in concentric spherical shells around the zero point. The current-function of each spherical layer is a spherical function of the same kind as the inducing; the current-curves have therefore the same form as the level-curves of the inducing potential; but they appear rotated towards them a certain

* Inaugural Dissertation: Berlin, 1880. 93 pp. Abstract by the Author.

angle. This amounts to 90° when self-induction is neglected; it is more than 90°, reckoned in the direction of the rotation, when the self-induction becomes sensible. The magnitude of this apparent rotation depends, when the hollow spheres are thin, in a very simple manner, on the velocity of rotation; so that thin hollow spheres are especially suited for experimental investigations. With hollow spheres of finite thickness the rotation is different for the different layers: the inner precede the outer; their rotation is bound to no limit, and increases to infinity when the velocity of rotation increases infinitely. The rotation of the outermost layer converges towards a fixed value. The intensity increases at the commencement with the rotation-velocity, but nowhere so rapidly as the latter; at greater velocities of rotation it diminishes again in the inner layers. With infinitely increasing rotation-velocities the intensity vanishes in the inner layers, and the entire phenomenon is condensed at the boundary of the sphere. The current that takes place here protects the interior from the external influence, apart from certain limitations.

The proof and the exact calculation of this course of the phenomenon forms the body of the investigation, to which are appended the following results:—

(1) If the self-induction be neglected, the total-current function can be represented in a closed form by the outer potential without any resolution of the latter according to spherical functions being necessary.

(2) Plane infinite disks are treated as portions of infinite hollow spheres. For these, taking account of the self-induction, developments are given which make the solution of the outer potential superfluous.

(3) The magnetic potential of the induced current, and the heat generated by it, are calculated, from which results the work requisite for maintaining the rotation, and the moment of rotation which the sphere gives to the external magnets about the rotation-axis.

(4) The calculation is extended to spheres whose mass is capable of receiving magnetic polarity.

(5) It is shown how the investigation can be extended to other rotation-bodies if the effect of self-induction be neglected. With this limitation, the problem is solved for a finite circular disk.

(6) Dielectric spheres are considered, on the assumption that in them the electrodynamic forces have the same action as equal electrostatic forces.

(7) The formulæ are applied to special cases, and the deductions illustrated by drawings. The cases are:—rectilinear motions of a pole over a plane plate at different velocities; an infinite and a finite disk rotating under the influence of rectilinear currents; very thin hollow spheres, and solid spheres, in a homogeneous magnetic field; stopping rotating conducting spheres by a suddenly excited electromagnet; damping in a galvanometer with a hollow spherical damper.—Wiedemann's *Beiblätter*, 1880, No. 8, pp. 622–624.

INDEX TO VOL. X.

END OF THE TENTH VOLUME.

ALERE FLAMMAM.

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7